





# MORE LETTERS OF CHARLES DARWIN

A RECORD OF HIS WORK  
IN A SERIES OF HITHERTO  
UNPUBLISHED LETTERS

EDITED BY FRANCIS DARWIN, FELLOW OF  
CHRIST'S COLLEGE, AND A. C. SEWARD, FELLOW  
OF EMMANUEL COLLEGE, CAMBRIDGE . . .

IN TWO VOLUMES  
ILLUSTRATED

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# CONTENTS OF VOLUME II

## CHAPTER VII

	PAGE
GEOGRAPHICAL DISTRIBUTION, 1867—1882 . . . . .	I

## CHAPTER VIII

MAN, 1860—1882 . . . . .	30
I. DESCENT OF MAN, 1860—1882 . . . . .	30
II. SEXUAL SELECTION, 1866—1872 . . . . .	56
III. EXPRESSION, 1868—1874 . . . . .	98

## CHAPTER IX

GEOLOGY, 1840—1882 . . . . .	113
I. VULCANICITY AND EARTH-MOVEMENTS, 1840—1881	113
II. ICE-ACTION, 1841—1882 . . . . .	148
III. THE PARALLEL ROADS OF GLEN ROY, 1841—1880	171
IV. CORAL REEFS, FOSSIL AND RECENT, 1841—1881 .	193
V. CLEAVAGE AND FOLIATION, 1846—1856 . . . .	199
VI. AGE OF THE WORLD, 1868—1877 . . . . .	211
VII. GEOLOGICAL ACTION OF EARTH-WORMS, 1880—1882	212
VIII. MISCELLANEOUS, 1846—1878 . . . . .	217

## CHAPTER X

BOTANY, 1843—1871 . . . . .	242
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## CHAPTER XI

	PAGE
SECTION I, 1863—1881 . . . . .	333
II. MISCELLANEOUS, 1863—1866 . . . . .	333
III. CORRESPONDENCE WITH FRITZ MÜLLER, 1865—1881	344
IV. MISCELLANEOUS, 1868—1881 . . . . .	371

## CHAPTER XII

SECTION I AND MISCELLANEOUS SUBJECTS, 1867—1882 . . . . .	435
II. MISCELLANEOUS, 1867—1882 . . . . .	435
III. MISCELLANEOUS SUBJECTS, 1867—1882 . . . . .	441

## ILLUSTRATIONS IN VOLUME II

CHARLES DARWIN, 1881 . . . . .	<i>Frontispiece</i>
From a photograph by ELLIOTT & FRY.	
ALFRED RUSSEL WALLACE, 1878 . . . . .	<i>To face page 12</i>
From a photograph by MAULL & FOX.	
GEORGE J. ROMANES, 1891 . . . . .	” ” ” 48
From a photograph by ELLIOTT & FRY. (Romanes' <i>Life</i> .)	
CHARLES LYELL . . . . .	” ” ” 113
From a photograph by MAULL & FOX. (Lyell's <i>Life</i> , Vol. II.)	
CHARLES DARWIN, 1854 (?) . . . . .	” ” ” 204
From a photograph by MAULL & FOX.	
JOSEPH DALTON HOOKER, 1891 . . . . .	” ” ” 242
From a photograph by HAWKER.	
FRITZ MÜLLER . . . . .	” ” ” 344
From a photograph.	

# CHARLES DARWIN

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## CHAPTER VII

### GEOGRAPHICAL DISTRIBUTION

1843—1882

(*Continued*)

J. D. Hooker to C. Darwin.

Let

Kew, Jan. 20th, 1867.

Prof. Miquel, of Utrecht, begs me to ask you for your carte, and offers his in return. I grieve to bother you on such a subject. I am sick and tired of this carte correspondence. I cannot conceive what Humboldt's Pyrenean violet is: no such is mentioned in Webb, and no alpine one at all. I am sorry I forgot to mention the stronger African affinity of the eastern Canary Islands. Thank you for mentioning it. I cannot admit, without further analysis, that most of the peculiar Atlantic Islands genera were derived from Europe, and have since become extinct there. I have rather thought that many are only altered forms of existing European genera; but this is a very difficult point, and would require a careful study of such genera and allies with this object in view. The subject has often presented itself to me as a grand one for analytic botany. No doubt its establishment would account for the community of the peculiar genera on the several groups and islets, but whilst so many species are common we must allow for a good deal of migra-



me of having suggested to me the  
of plants with irregular flowers is  
was a deuced deal too good an idea  
easily in my block, though I did not  
do so. No doubt your suggestion  
in some corner of my sensorium. I should  
not the point.

On the Island of Land amongst your volcanic islands I  
of a sailor who was wrecked on the  
a volcanic mountain and hot springs  
is called the *Wreck of the Favourite*.<sup>1</sup>

IV. 817.1

To J. D. Hooker.

Down, March 17th, 1867.

A long time since I have written, but I cannot boast  
I have retained ~~them~~ <sup>clearly</sup> towards you, but from  
a lot of work. . . . You ask what I have been doing,  
but blackening proofs with corrections. I do not  
in the English naturally writes so vile a style

In your paper on *Insular Floras* (p. 9) there is what I  
think an error, which I before pointed out to you: viz.  
you say that the plants which are wholly distinct from those  
of nearest continent are often very common<sup>2</sup> instead of very  
rare. Etty<sup>3</sup>, who has read your paper with great interest, was  
confounded by this sentence. By the way, I have stumbled

---

*Narrative of the Wreck of the "Favourite" on the Island of Desola-  
tion: detailing the Adventures, Sufferings and Privations of John Munn,  
an Historical Account of the Island and its Whale and Sea Fisheries*  
Edited by W. B. Clarke: London, 1850.

<sup>2</sup> *Insular Floras*, pamphlet reprinted from the *Gardeners' Chronicle*,  
p. 9: "As a general rule the species of the mother continent are propor-  
tionally the most abundant, and cover the greatest surface of the islands.  
The peculiar species are rarer, the peculiar genera of continental affinity  
are rarer still; whilst the plants having no affinity with those of the  
mother continent are often very common." In a letter of March 20th  
1867, Sir Joseph explains that in the case of the Atlantic islands it is  
the "peculiar genera of European affinity that are so rare" while

on two old notes: one, that twenty-two species of European Le birds occasionally arrive as chance wanderers to the Azores; and, secondly, that trunks of American trees have been known to be washed on the shores of the Canary Islands by the Gulf-stream, which returns southward from the Azores. What poor papers those of A. Murray are in *Gardeners' Chronicle*. What conclusions he draws from a single *Carabus*,<sup>1</sup> and that a widely ranging genus! He seems to me conceited; you and I are fair game geologically, but he refers to Lyell, as if his opinion on a geological point was worth no more than his own. I have just bought, but not read a sentence of, Murray's big book,<sup>2</sup> second-hand, for 30s., new, so I do not envy the publishers. It is clear to me that the man cannot reason. I have had a very nice letter from Scott at Calcutta<sup>3</sup>: he has been making some good observations on the acclimatisation of seeds from plants of same species, grown in different countries, and likewise on how far European plants will stand the climate of Calcutta. He says he is astonished how well some flourish, and he maintains, if the land were unoccupied, several could easily cross, spreading by seed, the Tropics from north to south, so he knows how to please me; but I have told him to be cautious, else he will have dragons down on him. . . .

As the Azores are only about two-and-a-half times more distant from America (in the same latitude) than from Europe, on the occasional migration view (especially as oceanic currents come directly from West Indies and Florida, and heavy gales of wind blow from the same direction), a large percentage of the flora ought to be American; as it is, we have only the *Sanicula*, and at present we have no explanation of this apparent anomaly, or only a feeble indication of an explanation in the birds of the Azores being all European.

---

<sup>1</sup> "Dr. Hooker on Insular Floras" (*Gardeners' Chronicle*, 1867, pp. 152, 181). The reference to the Carabidous beetle (*Aplothorax*) is at p. 181.

<sup>2</sup> *Geographical Distribution of Mammals*. 1866.

To J. D. Hooker.

Down, March 21st [1867].

Many thanks for your pleasant and very amusing letter. You have been treated shamefully by Etty and me, but now I know the facts, the sentence seems to me quite clear. As we have both blundered, it would be well to modify the sentence something as follows: "whilst, on the other hand, the plants which are related to those of distant continents, but have no affinity with those of the nearest continent are often very common." I forget whether I explain this circumstance, but it seems to me very mysterious. Do always remember that nothing in the world gives us so much pleasure as seeing you here whenever you can come. I chuckle over what you say of And. Murray must grapple with his book some day.

To C. Lyell.

Down, Oct. 31st [1867].

[P. Mansell] Weale sent to me from Natal a small packet of dry locust dung, under  $\frac{1}{2}$  oz., with the statement that it is believed that they introduce new plants into a district.<sup>2</sup> This statement, however, must be very doubtful. From this packet seven plants have germinated, belonging to at least two kinds of grasses. There is no error, for I dissected some of the seeds out of the middle of the pellets. It deserves notice that locusts are sometimes blown far out to sea. I caught one 370 miles from Africa, and I have heard of much greater distances. You might like to hear the following case, as it relates to a migratory bird belonging to the most wandering of all orders—viz. the woodcock.<sup>3</sup> The tarsus was firmly coated with mud, weighing when dry

<sup>1</sup> Sir Joseph Hooker wrote (March 23rd, 1867): "I see you 'smell a rat' in the matter of insular plants that are related to those of [a] distant continent being common. Yes, my beloved friend, let me make a clean breast of it. I only found it out after the lecture was in print! . . . I have been waiting ever since to 'think it out,' and write to you about it, coherently. I thought it best to squeeze it in, anyhow or anywhere, rather than leave so curious a fact . . ."

9 grains, and from this the *Juncus bufonius*, or toad rush, Lc  
germinated. By the way, the locust case verifies what I said  
in the *Origin*, that many possible means of distribution would  
be hereafter discovered. I quite agree about the extreme  
difficulty of the distribution of land mollusca. You will have  
seen in the last edition of *Origin*<sup>1</sup> that my observations on the  
effects of sea-water have been confirmed. I still suspect that  
the legs of birds which roost on the ground may be an efficient  
means ; but I was interrupted when going to make trials on  
this subject, and have never resumed it.

We shall be in London in the middle or latter part of  
November, when I shall much enjoy seeing you. Emma  
sends her love, and many thanks for Lady Lyell's note.

To J. D. Hooker.

Lc

Down, Wednesday [1867].

I daresay there is a great deal of truth in your remarks  
on the glacial affair, but we are in a muddle, and shall  
never agree. I am bigoted to the last inch, and will not  
yield. I cannot think how you can attach so much weight  
to the physicists, seeing how Hopkins, Hennessey, Haughton,  
and Thomson have enormously disagreed about the rate of  
cooling of the crust ; remembering Herschel's speculations  
about cold space,<sup>2</sup> and bearing in mind all the recent specula-  
tions on change of axis, I will maintain to the death that  
your case of Fernando Po and Abyssinia<sup>3</sup> is worth ten times

---

<sup>1</sup> *Origin*, Ed. iv., p. 429. The reference is to MM. Marten's experi-  
ments on seeds "in a box in the actual sea."

<sup>2</sup> The reader will find some account of Herschel's views in Lyell's  
*Principles*, 1872, Ed. xi., Vol. I., p. 283.

<sup>3</sup> See *Origin*, Ed. vi., p. 337 : "Dr. Hooker has also lately shown  
that several of the plants living on the upper parts of the lofty island of  
Fernando Po and on the neighbouring Cameroon mountains, in the Gulf  
of Guinea, are closely related to those in the mountains of Abyssinia,  
and likewise to those of temperate Europe." Darwin evidently means  
that such facts as these are better evidence of the gigantic periods of  
time occupied by evolutionary changes than the discordant conclusions  
of the physicists. See *Linn. Soc. Journ.*, Vol. VII., p. 180, for Hooker's  
general conclusions : also Hooker and Ball's *Morocco*, Appendix F, p. 421.

more than the temperate zone physicists. Your remarks on disregarding temperate plants and disregarding the tropical plants made me at first uncomfortable, but I soon recovered. You say that all botanists would agree that many tropical plants would not withstand a somewhat cooler climate. But I have come not to care at all for general beliefs without supporting facts. I have suffered too often from this: thus I found in every book the general statement that a host of flowers were fertilised in the bud, that seeds could not withstand salt water, etc., etc. I would far more trust such positive accounts as that by you of the mixed vegetation on the Himalayas and other such accounts. And with respect to tropical plants withstanding the slowly coming on cool periods, I trust to such facts as yours (and others) about seeds of two same species from mountains and plains having acquired a slightly different climatal constitution. I know all that I have said will excite in you savage contempt towards me. Do not answer this rigmarole, but attack me to your heart's content, and to that of mine, whenever you can come here, and may it be soon.

Ms. A. 9. 2. 10

J. D. Hooker to C. Darwin.

Kew, 1870.

The following extract from a letter of Sir J. D. Hooker shows the tables reversed between the correspondents.

Grove is disgusted at your being disquieted about W. Thomson. Tell George from me not to sit upon you with his mathematics. When I threatened your tropical cooling views with the facts of the physicists, you snubbed me and the facts sweetly, over and over again; and now, because a scarecrow

intimate relationship with Abyssinia, of whose flora it is a member, and from which it is separated by 1800 miles of absolutely unexplored country; (2) the curious relationship with the East African islands, which are still farther off; (3) the almost total dissimilarity from the Cape flora." For Sir J. D. Hooker's general conclusions on the Cameroon plants see *Linn. Soc. Journ.* VII., p. 180. More recently equally striking cases have come to light: for instance, the existence of a Mediterranean genus, *Adenocarpus*, in the Cameroons and on Kilima Njaro, and nowhere else in Africa: and the probable

of  $x+y$  has been raised on the selfsame facts, you boo-boo. Le  
Take another dose of Huxley's penultimate G. S. Address,<sup>1</sup>  
and send George back to college.

To J. D. Hooker.

Le

Feb. 3rd [1868].

I am now reading Miquel on *Flora of Japan*,<sup>2</sup> and like it: it is rather a relief to me (though, of course, not new to you) to find so very much in common with Asia. I wonder if A. Murray's<sup>3</sup> notion can be correct, that a [profound] arm of the sea penetrated the west coast of N. America, and prevented the Asiatico-Japan element colonising that side of the continent so much as the eastern side; or will climate suffice? I shall to the day of my death keep up my full interest in Geograph. Distribution, but I doubt whether I shall ever have strength to come in any fuller detail than in the *Origin* to this grand subject. In fact, I do not suppose any man could master so comprehensive a subject as it now has become, if all kingdoms of nature are included. I have read Murray's book, and am disappointed—though, as you said, here and there clever thoughts occur. How

---

<sup>1</sup> Huxley's Anniversary Address to the Geological Soc., 1869 (*Collected Essays*, VIII., p. 305). This is a criticism of Lord Kelvin's paper "On Geological Time" (*Trans. Geolog. Soc. Glasgow*, III.). At p. 336 Mr. Huxley deals with Lord Kelvin's "third line of argument, based on the temperature of the interior of the earth." This was no doubt the point most disturbing to Mr. Darwin, since it led Lord Kelvin to ask (as quoted by Huxley), "Are modern geologists prepared to say that all life was killed off the earth 50,000, 100,000, or 200,000 years ago?" Mr. Huxley, after criticising Lord Kelvin's data and conclusion, gives his conviction that the case against Geology has broken down. With regard to evolution, Huxley (p. 328) ingeniously points out a case of circular reasoning. "But it may be said that it is biology, and not geology, which asks for so much time—that the succession of life demands vast intervals; but this appears to me to be reasoning in a circle. Biology takes her time from geology. The only reason we have for believing in the slow rate of the change in living forms is the fact that they persist through a series of deposits which, geology informs us, have taken a long while to make. If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly."

...view not affording the least explanation  
of the numerous adaptations everywhere to be seen  
...does not in the least trouble his mind. One  
of the most curious cases which he adduces seems to me  
to be the two allied fresh-water, highly peculiar porpoises  
of the Ganges and Indus; and the more distantly allied  
porpoise of the Amazon. Do you remember his explanation  
of the arm of the sea becoming cut off, like the Caspian,  
and becoming fresh-water, and then divided into two  
basins, (perhaps) giving rise to two great rivers. But  
...thrown on the affinity of the Amazon form.  
...Flower's paper<sup>1</sup> that these fresh-water  
porpoises form two sub-families, making an extremely  
small and intermediate, very small family. Hence to us  
...clearly remnants of a large group; and I cannot  
...we have a good instance precisely like that of  
...of a large ancient marine group, preserved  
...in fresh-water, where there has been less  
...and consequently little modification.<sup>2</sup> What  
...fact that is which Miquel gives of the beech not  
...beyond the Caucasus, and then reappearing in  
Japan like your Himalayan *Pinus*,<sup>3</sup> and the cedar of  
Lebanon. I know of nothing that gives one such an idea  
of the recent mutations in the surface of the land as  
these living "outliers." In the geological sense we must,

<sup>1</sup> *Zoolog. Trans.* VI., 1869, p. 115. The toothed whales are divided  
into the *Physeteridae*, the *Delphinidae*, and the *Platanistidae*, which latter  
is placed between the two other families, and is divided into the sub-  
families *Iniinae* and *Platanistinae*.

<sup>2</sup> See Vol. I., p. 143, Letter 95.

<sup>3</sup> For *Pinus* read *Deodar*. The essential identity of the deodar and  
the cedar of Lebanon was pointed out in Hooker's *Himalayan Journals*  
in 1854 (Vol. I., p. 257.n). In the *Nat. History Review*, Jan., 1862,  
the question is more fully dealt with by him, and the distribution  
discussed. The nearest point at which cedars occur is the Bulgar-dagh  
chain of Taurus—250 miles from Lebanon. Under the name of *Cedrus*  
*atlantica* the tree occurs in mass on the borders of Tunis, and as  
Deodar it first appears to the east in the cedar forests of Afghanistan.  
Sir J. D. Hooker supposes that, during a period of greater cold, the cedars  
on the Taurus and on Lebanon lived many thousand feet nearer the  
sea-level and—

I suppose, admit that every yard of land has been Le successively covered with a beech forest between the Caucasus and Japan!

I have not yet seen (for I have not sent to the station) Falconer's works. When you say that you sigh to think how poor your reprinted memoirs would appear, on my soul I should like to shake you till your bones rattled for talking such nonsense. Do you sigh over the *Insular Floras*, the Introduction to New Zealand Flora, to Australia, your Arctic Flora, and dear Galapagos, etc., etc., etc.? In imagination I am grinding my teeth and choking you till I put sense into you. Farewell. I have amused myself by writing an audaciously long letter. By the way, we heard yesterday that George has won the second Smith's Prize, which I am excessively glad of, as the Second Wrangler by no means always succeeds. The examination consists exclusively of [the] most difficult subjects, which such men as Stokes, Cayley, and Adams can set.

A. R. Wallace to C. Darwin.

Le

March 8th, 1868.

. . . While writing a few pages on the northern alpine forms of plants on the Java mountains I wanted a few cases to refer to like Teneriffe, where there are no northern forms and scarcely any alpine. I expected the volcanoes of Hawaii would be a good case, and asked Dr. Seemann about them. It seems a man has lately published a list of Hawaiian plants, and the mountains swarm with European alpine genera and some species!<sup>1</sup> Is not this most extraordinary, and a puzzler? They are, I believe, truly oceanic islands, in the absence of mammals and the extreme poverty of birds and insects, and they are within the Tropics.

Will not that be a hard nut for you when you come to treat in detail on geographical distribution? I enclose Seemann's note, which please return when you have copied the list, if of any use to you.



To J. D. Hooker.

Down, Feb. 21st [1870].

I read yesterday the notes on Round Island<sup>1</sup> which I owe to you. Was there ever such an enigma? If, in the course of a week or two, you can find time to let me hear what you think, I should very much like to hear: or we hope to be at Erasmus on March 4th for a week. Would there be any chance of your coming to luncheon then? What a case it is. Palms, screw-pines, four snakes—not one being in main island—lizards, insects, and not one land bird. But above everything, such a proportion of *monocotyledons*! The conditions do not seem very different from the Tuff Galapagos Island, but, as I must remember, very few *monocotyledons* there. Then, again, the island seems to have been elevated. I wonder whether it stands out in the line of any oceanic current, which does not so forcibly strike the main island? But why, oh, why should so many *monocotyledons* have come there? or why should they have survived there more than on the main island, if once connected? So, again, I cannot conceive that four snakes should have become extinct in Mauritius and survived on Round Island. For a moment I thought that Mauritius might be the newer island, but the enormous degradation which the outer ring of rocks has undergone flatly contradicts this, and the marine remains on the summit of Round Island indicate the island to be comparatively new—unless, indeed, they are fossil and extinct marine remains. Do tell me what you think. There never was such an enigma. I rather lean to separate immigration, with, of course, subsequent modification; some forms, of

<sup>1</sup> In Wallace's *Island Life*, p. 410, Round Island is described as an islet "only about a mile across, and situated about fourteen miles north-east of Mauritius." Wallace mentions a snake, a python belonging to the peculiar and distinct genus *Casarea*, as found on Round Island, and nowhere else in the world. The palm *Latania Loddigesii* is quoted by Wallace as "confined to Round Island and two other adjacent islets." See Baker's *Flora of the Mauritius and the Seychelles*. Mr. Wallace says that, judging from the soundings, Round Island was connected with Mauritius and ...

course, also coming from Mauritius. Speaking of Mauritius Let reminds me that I was so much pleased the day before yesterday by reading a review of a book on the geology of St. Helena, by an officer who knew nothing of my hurried observations, but confirms nearly all that I have said on the general structure of the island, and on its marvellous denudation. The geology of that island was like a novel.

To A. Blytt.<sup>1</sup>

Let

Down, March 28th, 1876.

The following refers to Blytt's *Essay on the Immigration of the Norwegian Flora during Alternating Rainy and Dry Periods*, Christiania, 1876.

I thank you sincerely for your kindness in having sent me your work on the *Immigration of the Norwegian Flora*, which has interested me in the highest degree. Your view, supported as it is by various facts, appears to me the most important contribution towards understanding the present distribution of plants, which has appeared since Forbes' essay on the effects of the Glacial Period.

To Aug. Forel.

Let

Down, June 19th, 1876.

I hope you will allow me to suggest an observation, should any opportunity occur, on a point which has interested me for many years—viz., how do the coleoptera which inhabit the nests of ants colonise a new nest?<sup>2</sup> Mr. Wallace, in reference to the presence of such coleoptera in Madeira, suggests that their ova may be attached to the winged female ants, and that these are occasionally blown across the ocean to the island. It would be very interesting to

---

<sup>1</sup> Axel Gudbrand Blytt (1843-98), the son of the well-known systematist M. N. Blytt. He was attached to the Christiania Herbarium in 1865, and in 1880 became Professor of Botany in the University. His best-known work is the essay referred to above, but he was also known for purely systematic work in Botany as well as for meteorological and geological contributions to science. The above facts are taken from C. Holtermann's obituary notice in the *Reichte des Deutschen Bot. Gesells. 1898*.

...are guided by instinct to attach them to  
...or whether the larvæ pass through an  
...*Sitaris* or *Maloe*, or cling to the bodies  
...obviously requires no answer.  
...continue your most interesting investigations

To A. R. Wallace.<sup>2</sup>

My letters refer to Mr. Wallace's *Geographical Distribution*, 1876.

[Hopedene]<sup>3</sup>, June 5th, 1876.

I have the pleasure of expressing to you my  
...of your book,<sup>4</sup> though I have read only  
...having been to do as little as  
...I feel sure that you have laid a  
...foundation for all future work on Distribution.  
...will be to see hereafter plants treated in  
...your views; and then all insects, pulmonate  
...and fresh-water fishes, in greater detail than I  
...suppose you have given to these lower animals. The point  
...has interested me most, but I do not say the most  
...point, is your protest against sinking imaginary  
...in a quite reckless manner, as was stated by  
...followed, alas, by Hooker, and caricatured by  
...[Andrew] Murray! By the way, the main  
...impression that the latter author has left on my mind is his  
...utter want of all scientific judgment. I have lifted up my  
...voice against the above view with no avail, but I have no

Dr. Sharp is good enough to tell us that he is not aware of any such  
adaptation. Broadly speaking, the distribution of the nest-inhabiting  
beetles is due to co-migration with the ants, though in some cases the  
ants transport the beetles. *Sitaris* and *Maloe* are beetles which live "at  
the expense of bees of the genus *Anthophora*." The eggs are laid not in  
but near the bees' nest; in the early stage the larva is active and has the  
instinct to seize any hairy object near it, and in this way they are carried by  
the *Anthophora* to the nest. Dr. Sharp states that no such preliminary  
stage is known in the ant's-nest beetles. For an account of *Sitaris* and  
*Maloe*, see Sharp's *Insects*, II., p. 272.

<sup>2</sup> Published in *Life and Letters*...



*From a photograph by Maull & Fox*

MR. A. R. WALLACE

doubt that you will succeed, owing to your new arguments Let and the coloured chart. Of a special value, as it seems to me, is the conclusion that we must determine the areas, chiefly by the nature of the mammals. When I worked many years ago on this subject, I doubted much whether the now-called Palæarctic and Nearctic regions ought to be separated; and I determined if I made another region that it should be Madagascar. I have, therefore, been able to appreciate your evidence on these points. What progress Palæontology has made during the last twenty years! but if it advances at the same rate in the future, our views on the migration and birthplace of the various groups will, I fear, be greatly altered. I cannot feel quite easy about the Glacial period, and the extinction of large mammals, but I must hope that you are right. I think you will have to modify your belief about the difficulty of dispersal of land molluscs; I was interrupted when beginning to experimentise on the just hatched young adhering to the feet of ground-roosting birds. I differ on one other point—viz. in the belief that there must have existed a Tertiary Antarctic continent, from which various forms radiated to the southern extremities of our present continents. But I could go on scribbling for ever. You have written, as I believe, a grand and memorable work, which will last for years as the foundation for all future treatises on Geographical Distribution.

P.S.—You have paid me the highest conceivable compliment, by what you say of your work in relation to my chapters on distribution in the *Origin*, and I heartily thank you for it.

From A. R. Wallace to C. Darwin.

Le

The Dell, Grays, Essex, June 7th, 1876.

Many thanks for your very kind letter. So few people will read my book at all regularly, that a criticism from one who does so will be very welcome. If, as I suppose, it is only to p. 184 of Vol. I. that you have read, you cannot yet quite see my conclusions on the points you refer to (land molluscs and Antarctic continent). My own conclusion fluctuated during the progress of the book, and I have I

as making South America with Australia or New Zealand  
as you will see at Vol. I., pp. 398-403, and 459-66. My  
general conclusions as to distribution of land mollusca<sup>1</sup> are  
in Vol. II., pp. 524-5. When you have read these passages  
and looked at the general facts which lead to them, I shall  
be glad to hear if you still differ from me.

Although, of course, present results as to the origin and  
migrations of genera of mammals will have to be modified  
on new discoveries, I cannot help thinking that much  
will remain unaffected, because in all geographical and  
geological discoveries the great outlines are soon reached,  
the details alone remain to be modified. I also think much  
of the geological evidence is now so accordant with, and  
confirmatory of, Geographical Distribution, that it is *prima*  
facti correct in outline. Nevertheless, such vast masses of  
new facts will come out in the next few years that I quite  
anticipate the labour of incorporating them in a new edition.

I hope your health is improved; and when, quite at your  
leisure, you have waded through my book, I trust you will  
again let me have a few lines of friendly criticism and advice.

To A. R. Wallace.

Down, June 17th, 1876.

I have now finished the whole of Vol. I., with the same  
interest and admiration as before; and I am convinced that  
my judgment was right and that it is a memorable book,  
the basis of all future work on the subject. I have nothing  
particular to say, but perhaps you would like to hear my  
impressions on two or three points. Nothing has struck  
me more than the admirable and convincing manner in  
which you treat Java. To allude to a very trifling point,  
it is capital about the unadorned head of the Argus-pheasant.<sup>2</sup>

<sup>1</sup> *Geogr. Distrib.*, II., pp. 524, 525. Mr. Wallace points out that  
"hardly a small island on the globe but has some land-shells peculiar to  
it"—and he goes so far as to say that probably air-breathing mollusca  
have been chiefly distributed by air- or water-carriage, rather than by  
voluntary dispersal on the land.

<sup>2</sup> See *Descent of Man* Ed. 1, pp. 100, 101.

How plain a thing is, when it is once pointed out! What Lett  
a wonderful case is that of Celebes: I am glad that you  
have slightly modified your views with respect to Africa.<sup>1</sup>  
And this leads me to say that I cannot swallow the so-called  
continent of Lemuria—*i.e.*, the direct connection of Africa  
and Ceylon.<sup>2</sup> The facts do not seem to me many and  
strong enough to justify so immense a change of level.  
Moreover, Mauritius and the other islands appear to me  
oceanic in character. But do not suppose that I place my  
judgment on this subject on a level with yours. A wonder-  
fully good paper was published about a year ago on India,  
in the *Geolog. Journal*, I think by Blanford.<sup>3</sup> Ramsay  
agreed with me that it was one of the best published for a  
long time. The author shows that India has been a continent

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little lectures which on rare occasions he would give to a visitor interested  
in Natural History. In Mr. Wallace's book the meaning of the ocelli  
comes in by the way, in the explanation of Plate IX., "A Malayan Forest  
with some of its peculiar Birds." Mr. Wallace (vol. i., p. 340) points out  
that the head of the Argus pheasant is, during the display of the wings,  
concealed from the view of a spectator in front, and this accounts for the  
absence of bright colour on the head—a most unusual point in a pheasant.  
The case is described as a "remarkable confirmation of Mr. Darwin's  
views, that gaily coloured plumes are developed in the male bird for the  
purpose of attractive display." For the difference of opinion between the  
two naturalists on the broad question of coloration see *Life and Letters*,  
III., p. 123. See Letters 440-453, pp. 72 *et seq.*

<sup>1</sup> "I think this must refer to the following passage in *Geog. Dist. of  
Animals*, Vol. I., pp. 286-7. 'At this period (Miocene) Madagascar was  
no doubt united with Africa, and helped to form a great southern continent  
which must at one time have extended eastward as far as Southern India  
and Ceylon; and over the whole of this the lemurine type no doubt  
prevailed.' At the time this was written I had not paid so much attention  
to islands, and in my *Island Life* I have given ample reasons for my  
belief that the evidence of extinct animals does not require any direct  
connection between Southern India and Africa."—Note by Mr. Wallace.

<sup>2</sup> See *Geog. Dist.*, I., p. 76. The name *Lemuria* was proposed by Mr.  
Sclater for an imaginary submerged continent extending from Madagascar  
to Ceylon and Sumatra. Mr. Wallace points out that if we confine our-  
selves to facts Lemuria is reduced to Madagascar, which he makes a  
subdivision of the Ethiopian Region.

<sup>3</sup> H. F. Blanford "On the Age and Correlations of the Plant-bearing

the present day. If I remember right, he believes in a former connection with *South Africa*.

I read, some twenty to thirty years ago, an account of teeth of *Mastodon*<sup>1</sup> found in the state of *Illinois*; but the statement may have been an error.

In respect to what you say about the colonising of *New Zealand*, I somewhere have an account of a frog frozen beneath of a Swiss glacier, and which revived when thawed. I may add that there is an Indian toad which can resist winter and hibernates the seaside. Nothing ever astonished me more than the case of the *Galaxias*<sup>2</sup>; but it does not seem known whether it may not be a migratory fish like the *Salmon*.

To A. R. Wallace.

Down, June 25th, 1876.

I have been able to read rather more quickly of late, and have finished your book. I have not much to say. Your account of the temperate parts of South America interested me much, and all the more from knowing something of the country. I like also much the general remarks towards the end of the volume on the land molluscs. Now for a few criticisms.

P. 122.<sup>3</sup>—I am surprised at your saying that "during the whole Tertiary period North America was zoologically far more strongly contrasted with South America than it is now." But we know hardly anything of the latter except during the Pliocene period; and then the mastodon, horse, several great edentata, etc., etc., were common to the north and south. If

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continent. Since the publication of Blanford's paper, much literature has appeared dealing with the evidence furnished by fossil plants, etc., in favour of the existence of a vast southern continent.

<sup>1</sup> In a letter to Falconer (Letter 155), Jan. 5th, 1863, Darwin refers to the supposed occurrence of *Mastodon* as having been "smashed" by Falconer.

<sup>2</sup> The only genus of the *Galaxiidae*, a family of fresh-water fishes occurring in New Zealand, Tasmania, and Terra del Fuego, ranging north as far as Queensland and Chile.



you are right, I erred greatly in my *Journal*, where I insisted on the former close connection between the two. Lette:

P. 252 and elsewhere.—I agree thoroughly with the general principle that a great area with many competing forms is necessary for much and high development; but do you not extend this principle too far—I should say much too far, considering how often several species of the same genus have been developed on very small islands?

P. 265.—You say that the Sittidæ extend to Madagascar, but there is no number in the tabular heading. [The number (4) was erroneously omitted.—A. R. W.]

P. 359.—*Rhinochetus* is entered in the tabular heading under No. 3 of the neotropical subregions. [An error: should have been the Australian.—A. R. W.]

Reviewers think it necessary to find some fault; and if I were to review you, the sole point which I should blame is your not giving very numerous references. These would save whoever follows you great labour. Occasionally I wished myself to know the authority for certain statements, and whether you or somebody else had originated certain subordinate views. Take the case of a man who had collected largely on some island, for instance St. Helena, and who wished to work out the geographical relations of his collections: he would, I think, feel very blank at not finding in your work precise references to all that had been written on St. Helena. I hope you will not think me a confoundedly disagreeable fellow.

I may mention a capital essay which I received a few months ago from Axel Blytt<sup>1</sup> on the distribution of the plants of Scandinavia; showing the high probability of there having been secular periods alternately wet and dry, and of the important part which they have played in distribution.

I wrote to Forel,<sup>2</sup> who is always at work on ants, and told him your views about the dispersal of the blind coleoptera, and asked him to observe.

I spoke to Hooker about your book, and feel sure that he would like nothing better than to consider the distribution of

whether he should ever have time.

I have now done my jottings, and once again cc  
state you on having brought out so grand a work.  
I have been a little disappointed at the review in *Nature*.<sup>1</sup>

A. R. Wallace to C. Darwin.

Rosehill, Dorking, July 23rd, 1876.

I should have replied sooner to your last kind an  
writing letters, but they reached me in the midst of n  
packing previous to removal here, and I have only just n  
of my Books and papers in a get-at-able state.

And first, many thanks for your close observation  
detecting the two absurd mistakes in the tabular heading  
As to the former greater distinction of the North an  
South American faunas, I think I am right. The edentat  
being proved (as I hold) to have been mere temporar  
migrants into North America in the post-Pliocene epoc  
no part of its Tertiary fauna. Yet in South Americ  
were so enormously developed in the Pliocene epoc  
that we know, if there is any such thing as evolution, etc  
that strange ancestral forms must have preceded them i  
Miocene times.

*Mastodon*, on the other hand, represented by one or tw  
species only, appears to have been a late immigrant int  
South America from the north.

The immense development of ungulates (in varie  
families, genera, and species) in North America during th  
whole Tertiary epoch is, however, the great feature whic  
assimilates it to Europe, and contrasts it with South Americ  
True camels, hosts of hog-like animals, true rhinoceroses  
and hosts of ancestral horses, all bring the North America  
[fauna] much nearer to the Old World than it is now  
Even the horse, represented in all South America by *Equu*  
only, was probably a temporary immigrant from the north.

As to extending too far the principle (yours) of th  
necessity of comparatively large areas for the developmen  
of varied faunas, I may have done so, but I think not  
There is, I think, every probability that

where a varied fauna now exists, have been once more Letter extensive—*e.g.*, New Zealand, Madagascar: where there is no such evidence (*e.g.*, Galapagos), the fauna is very restricted.

Lastly, as to want of references: I confess the justice of your criticism; but I am dreadfully unsystematic. It is my first large work involving much of the labour of others. I began with the intention of writing a comparatively short sketch, enlarged it, and added to it bit by bit; remodelled the tables, the headings, and almost everything else, more than once, and got my materials in such confusion that it is a wonder it has not turned out far more crooked and confused than it is. I, no doubt, ought to have given references; but in many cases I found the information so small and scattered, and so much had to be combined and condensed from conflicting authorities, that I hardly knew how to refer to them or where to leave off. Had I referred to all authors consulted for every fact, I should have greatly increased the bulk of the book, while a large portion of the references would be valueless in a few years, owing to later and better authorities. My experience of referring to references has generally been most unsatisfactory. One finds, nine times out of ten, the fact is stated, and nothing more; or a reference to some third work not at hand!

I wish I could get into the habit of giving chapter and verse for every fact and extract; but I am too lazy, and generally in a hurry, having to consult books against time, when in London for a day.

However, I will try to do something to mend this matter, should I have to prepare another edition.

I return you Forel's letter. It does not advance the question much; neither do I think it likely that even the complete observation he thinks necessary would be of much use, because it may well be that the ova, or larvæ, or imagos of the beetles are not carried systematically by the ants, but only occasionally, owing to some exceptional circumstances. This might produce a great effect in distribution, yet be so rare as never to come under observation.

Several of your remarks in previous letters I shall carefully consider. I know that, compared with the extent

at the risk of having to alter my views in many points. I was so overwhelmed with zoological details, that I never went through the *Geological Society's Journal* as I ought to have done, and as I mean to do before writing more on the subject.

To F. Buchanan White.<sup>1</sup>

Down, Sept. 23rd [1878].

I have now read your paper, and I hope that you will not think me presumptuous in writing another line to say how excellent it seems to me. I believe that you have largely solved the problem of the affinities of the inhabitants of this most interesting little island, and this is a delightful triumph.

To J. D. Hooker.

Down, July 22nd [1879].

I have just read Ball's Essay.<sup>2</sup> It is pretty bold. The development as far as we can judge of all the higher plants within recent geological times is an abominable

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<sup>1</sup> "Written in acknowledgment of a copy of a paper (published by me in the *Proceedings of the Zool. Soc.*) on the Hemiptera of St. Helena but discussing the origin of the whole fauna and flora of that island."—F. B. W.

<sup>2</sup> The late John Ball's lecture "On the Origin of the Flora of the Alps" in the *Proceedings of the R. Geogr. Soc.*, 1879. Ball argues (p. 18) that "during ancient Palæozoic times, before the deposition of the Coal-measures, the atmosphere contained twenty times as much carbonic acid gas and considerably less oxygen than it does at present." He further assumes that in such an atmosphere the percentage of CO<sub>2</sub> in the higher mountains would be excessively different from that at the sea-level, and appends the result of calculations which gives the amount of CO<sub>2</sub> at the sea-level as 100 per 10,000 by weight, at a height of 10,000 feet as 12½ per 10,000. Darwin understands him to mean that the Vascular Cryptogams and Gymnosperms could stand the sea-level atmosphere, whereas the Angiosperms would only be able to exist in the higher regions where the percentage of CO<sub>2</sub> was small. It is not clear to us that Ball relies so largely on the condition of the atmosphere as regards CO<sub>2</sub>. If he does he is clearly in error, for everything we know of assimilation points to the conclusion that 100 per 10,000 (1 per cent.) is by no means a hurtful amount of CO<sub>2</sub> and that

mystery. Certainly it would be a great step if we could believe that the higher plants at first could live only at a high level; but until it is experimentally [proved] that Cycadeæ, ferns, etc., can withstand much more carbonic acid than the higher plants, the hypothesis seems to me far too rash. Saprota believes that there was an astonishingly rapid development of the high plants, as soon [as] flower-frequenting insects were developed and favoured intercrossing. I should like to see this whole problem solved. I have fancied

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avoid it. Ball draws attention to the imperfection of our plant records as regards the floras of mountain regions. It is, he thinks, conceivable that there existed a vegetation on the Carboniferous mountains of which no traces have been preserved in the rocks. See *Fossil Plants as Tests of Climate*, p. 40, A. C. Seward, 1892.

Since the first part of this note was written, a paper has been read (May 29th, 1902) by Dr. H. T. Brown and Mr. F. Escombe, before the Royal Society on "The Influence of varying amounts of Carbon Dioxide in the Air on the Photosynthetic Process of Leaves, and on the Mode of Growth of Plants." The author's experiments included the cultivation of several dicotyledonous plants in an atmosphere containing in one case 180 to 200 times the normal amount of  $\text{CO}_2$ , and in another between three and four times the normal amount. The general results were practically identical in the two sets of experiments. "All the species of flowering plants, which have been the subject of experiment, appear to be accurately 'tuned' to an atmospheric environment of three parts of  $\text{CO}_2$  per 10,000, and the response which they make to slight increases in this amount are in a direction altogether unfavourable to their growth and reproduction." The assimilation of carbon increases with the increase in the partial pressure of the  $\text{CO}_2$ . But there seems to be a disturbance in metabolism, and the plants fail to take advantage of the increased supply of  $\text{CO}_2$ . The authors say:—"All we are justified in concluding is, that if such atmospheric variations have occurred since the advent of flowering plants, they must have taken place so slowly as never to outrun the possible adaptation of the plants to their changing conditions."

Prof. Farmer and Mr. S. E. Chandler gave an account, at the same meeting of the Royal Society, of their work "On the Influence of an Excess of Carbon Dioxide in the Air on the Form and Internal Structure of Plants." The results obtained were described as differing in a remarkable way from those previously recorded by Téodoresco (*Rev. Gen. Botanique*, II., 1899).

It is hoped that Dr. Horace Brown and Mr. Escombe will extend

perhaps there was during long ages a small isolated continent in the S. Hemisphere which served as the birth place of the higher plants—but this is a wretchedly poor conjecture. It is odd that Ball does not allude to the obvious fact that there must have been alpine plants before the Glacial period, many of which would have returned to the mountains after the Glacial period, when the climate again became warm. I always accounted to myself in this manner for the gentians, etc.

Ball ought also to have considered the alpine insect common to the Arctic regions. I do not know how it may be with you, but my faith in the glacial migration is not at all shaken.

A. R. Wallace to C. Darwin.

This letter is in reply to Mr. Darwin's criticisms on Mr. Wallace *Geogr. Life*, 1880.

Pen-y-Bryn, St. Peter's Road, Croydon, Nov. 8th, 1880.

Many thanks for your kind remarks and notes on my book. Several of the latter will be of use to me if I have to prepare second edition, which I am not so sure of as you seem to be.

1. In your remark as to the doubtfulness of paucity of fossils being due to coldness of water, I think you overlook that I am speaking only of water in the latitude of the Alps in Miocene and Eocene times, when icebergs and glaciers temporarily descended into an otherwise warm sea; my theory being that there was no Glacial epoch at that time, but merely a local and temporary descent of the snow-line and glaciers owing to high excentricity and winter in aphelion.

2. I cannot see the difficulty about the cessation of the Glacial period.

Between the Miocene and the Pleistocene periods geographical changes occurred which rendered a true Glacial period possible with high excentricity. When the high excentricity passed away the Glacial epoch also passed away in the temperate zone; but it persists in the arctic zone, where, during the Miocene, there were mild climates, and this is due to the persistence of the changed geographical conditions. The present arctic climate is the result of the

As to "epoch" and "period," I use them as synonyms to lette avoid repeating the same word.

3. Rate of deposition and geological time. Here no doubt I may have gone to an extreme, but my "28 million years" may be anything under 100 millions, as I state. There is an enormous difference between mean and maximum denudation and deposition. In the case of the great faults the upheaval along a given line would itself facilitate the denudation (whether sub-aerial or marine) of the upheaved portion at a rate perhaps a hundred times above the average, just as valleys have been denuded perhaps a hundred times faster than plains and plateaux. So local subsidence might itself lead to very rapid deposition. Suppose a portion of the Gulf of Mexico, near the mouths of the Mississippi, were to subside for a few thousand years, it might receive the greater portion of the sediment from the whole Mississippi valley, and thus form strata at a very rapid rate.

4. You quote the Pampas thistles, etc., against my statement of the importance of preoccupation. But I am referring especially to St. Helena, and to plants naturally introduced from the adjacent continents. Surely if a certain number of African plants reached the island, and became modified into a complete adaptation to its climatic conditions, they would hardly be expelled by other African plants arriving subsequently. They might be so, conceivably, but it does not seem probable. The cases of the Pampas, New Zealand, Tahiti, etc., are very different, where highly developed aggressive plants have been artificially introduced. Under nature it is these very aggressive species that would first reach any island in their vicinity, and, being adapted to the island and colonising it thoroughly, would then hold their own against other plants from the same country, mostly less aggressive in character.

I have not explained this so fully as I should have done in the book. Your criticism is therefore useful.

5. My Chapter XXIII. is no doubt very speculative, and I cannot wonder at your hesitating at accepting my views. To me, however, your theory of hosts of existing species migrating over the tropical lowlands from the N. temperate to the S. temperate zone appears more speculative and more

Cape flora, which, if the temperature of tropical Africa had been so recently lowered, would certainly have spread northwards, and on the return of the heat could hardly have been driven back into the sharply defined and very restricted area in which it now exists.

As to the migration of plants from mountain to mountain not being so probable, as to remote islands, I think that is fully counterbalanced by two considerations:—

1. The area and abundance of the mountain stations along such a range as the Andes are immensely greater than those of the islands in the N. Atlantic, for example.

2. The temporary occupation of mountain stations by migrating plants (which I think I have shown to be probable) renders time a much more important element in increasing the number and variety of the plants so dispersed than in the case of islands, where the flora soon acquires a fixed and endemic character, and where the number of species is necessarily limited.

No doubt direct evidence of seeds being carried great distances through the air is wanted, but I am afraid can hardly be obtained. Yet I feel the greatest confidence that they are so carried. Take, for instance, the two peculiar orchids of the Azores (*Habenaria* sp.) What other mode of transit is conceivable? The whole subject is one of great difficulty, but I hope my chapter may call attention to a hitherto neglected factor in the distribution of plants.

Your references to the Mauritius literature are very interesting, and will be useful to me; and I again thank you for your valuable remarks.

Letter 397

To J. D. Hooker.

The following letters were written to Sir J. D. Hooker when he was preparing his Address as President of the Geographical Section of the British Association at its fiftieth meeting, at York. The second letter (August 12th) refers to an earlier letter of August 6th, published in *Life and Letters*, III., p. 246.

4, Bryanston Street, W., Saturday, 26th [Feb., 1881].

I should think that you might make a very interesting address on Geographical Distribution.



history *in petto* ; but I can see one very great difficulty—that Letter you yourself ought to figure most prominently in it ; and this you would not do, for you are just the man to treat yourself in a dishonourable manner. I should very much like to see you discuss some of Wallace's views, especially his ignoring the all-powerful effects of the Glacial period<sup>1</sup> with respect to alpine plants. I do not know what you think, but it appears to me that he exaggerates enormously the influence of débâcles or slips and new surface of soil being exposed for the reception of wind-blown seeds. What kinds of seeds have the plants which are common to the distant mountain-summits in Africa ? Wallace lately wrote to me about the mountain plants of Madagascar<sup>2</sup> being the same with those on mountains in Africa, and seemed to think it proved dispersal by the wind,

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<sup>1</sup> " Having been kindly permitted by Mr. Francis Darwin to read this letter, I wish to explain that the above statement applies only to my rejection of Darwin's view that the presence of arctic and north temperate plants in the *southern hemisphere* was brought about by the lowering of the temperature of the tropical regions during the Glacial period, so that even 'the lowlands of these great continents were everywhere tenanted under the equator by a considerable number of temperate forms' (*Origin of Species*, Ed. VI., p. 338). My own views are fully explained in Chapter XXIII. of my *Island Life*, published in 1880. I quite accept all that Darwin, Hooker, and Asa Gray have written about the effect of the Glacial epoch in bringing about the present distribution of alpine and arctic plants in the *northern hemisphere*."—Note by Mr. Wallace.

<sup>2</sup> The affinity with the flora of the Eastern African islands was long ago pointed out by Sir J. D. Hooker, *Linn. Soc. Journal*, VI., 1861, p. 3. Speaking of the plants of Clarence Peak in Fernando Po, he says, "The next affinity is with Mauritius, Bourbon, and Madagascar : of the whole 76 species, 16 inhabit these places and 8 more are closely allied to plants from there. Three temperate species are peculiar to Clarence Peak and the East African islands. . . ." The facts to which Mr. Wallace called Darwin's attention are given by Mr. J. G. Baker in *Nature*, Dec. 9th, 1880, p. 125. He mentions the Madagascar *Viola*, which occurs elsewhere only at 7,000 ft. in the Cameroons, at 10,000 ft. in Fernando Po and in the Abyssinian mountains ; and the same thing is true of the Madagascar *Geranium*. In Mr. Wallace's letter to Darwin, dated Jan. 1st, 1881, he evidently uses the expression "passing through the air" in contradistinction to the migration of a species by gradual extension of its area on land. "Through the air" would moreover include occasional modes

Letter 397 apparently having inquired what sorts of seeds the plants bore.

I suppose it would be travelling too far (though for the geographical section the discussion ought to be far-reaching) but I should like to see the European or northern element in the Cape of Good Hope flora discussed. I cannot swallow Wallace's view that European plants travelled down the Andes, tenanted the hypothetical Antarctic continent (in which I quite believe), and thence spread to South Australia and the Cape of Good Hope.

Moseley told me not long ago that he proposed to search at Kerguelen Land the coal beds most carefully, and was absolutely forbidden to do so by Sir W. Thomson, who said that he would undertake the work, and he never once visited them. This puts me in a passion. I hope that you will keep to your intention and make an address on distribution. Though I differ so much from Wallace, his *Island Life* seems to me a wonderful book.

Farewell. I do hope that you may have a most prosperous journey. Give my kindest remembrances to Asa Gray.

Letter 398

To J. D. Hooker.

Down, Aug. 12th, 1881.

... I think that I must have expressed myself badly about Humboldt. I should have said that he was more remarkable for his astounding knowledge than for originality. I have always looked at him as, in fact, the founder of the geographical distribution of organisms. I thought that I had read that extinct fossil plants belonging to Australian forms had lately been found in Australia, and all such cases seem to me very interesting, as bearing on development.

I have been so astonished at the apparently sudden coming in of the higher phanerogams, that I have sometimes fancied that development might have slowly gone on for an immense period in some isolated continent or large island, perhaps near the South Pole. I poured out my idle thoughts in writing, as if I had been talking with you.

No fact has so interested me for a heap of years as

analogous cases on the mountains of Madagascar.<sup>1</sup> . . . I Lette think that you ought to allude to these cases.

I most fully agree that no problem is more interesting than that of the temperate forms in the southern hemisphere, common to the north. I remember writing about this after Wallace's book appeared, and hoping that you would take it up. The frequency with which the drainage from the land passes through mountain-chains seems to indicate some general law—viz., the successive formation of cracks and lines of elevation between the nearest ocean and the already upraised land ; but that is too big a subject for a note.

I doubt whether any insects can be shown with any probability to have been flower feeders before the middle of the Secondary period. Several of the asserted cases have broken down.

Your long letter has stirred many pleasant memories of long past days, when we had many a discussion and many a good fight.

To J. D. Hooker.

Lette

Down, Aug. 21st, 1881.

I cannot aid you much, or at all. I should think that no one could have thought on the modification of species without thinking of representative species. But I feel sure that no discussion of any importance had been published on this subject before the *Origin*, for if I had known of it I should assuredly have alluded to it in the *Origin*, as I wished to gain support from all quarters. I did not then know of Von Buch's view (alluded to in my Historical Introduction in all the later editions). Von Buch published his *Isles Canaries* in 1836, and he here briefly argues that plants spread over a continent and vary, and the varieties in time come to be species. He also argues that closely allied species have been thus formed in the *separate* valleys of the Canary Islands, but not on the upper and open parts. I could lend you Von Buch's book, if you like. I have just consulted the passage.

I have not Baer's papers : but, as far as I remember, the

... agree about Wallace's position on the ocean and  
continent question.

... To return to geographical distribution: As far as I know  
no one ever discussed the meaning of the relation between  
representative species before I did, and, as I suppose, Wallace  
did in his paper before the Linnean Society. Von Buch's is  
the nearest approach to such discussion known to me.

To W. D. Crick.

The following letters are interesting not only for their own sake, but  
because they tell the history of the last of Mr. Darwin's publications—his  
letter to *Nature* on the "Dispersal of Freshwater Bivalves," April 6th, 1882

Down, Feb. 21st, 1882.

Your fact is an interesting one, and I am very much  
obliged to you for communicating it to me. You speak a  
little doubtfully about the name of the shell, and it would be  
dispensable to have this ascertained with certainty. Do  
you know any good conchologist in Northampton who could  
name it? If so I should be much obliged if you would inform  
me of the result.

Also the length and breadth of the shell, and how much  
of leg (which leg?) of the *Dytiscus* [a large water-beetle] has  
been caught. If you cannot get the shell named I could take  
it to the British Museum when I next go to London; but  
this probably will not occur for about six weeks, and you may  
object to lend the specimen for so long a time.

I am inclined to think that the case would be worth  
communicating to *Nature*.

P.S.—I suppose that the animal in the shell must have  
been alive when the *Dytiscus* was captured, otherwise the  
adductor muscle of the shell would have relaxed and the shell  
dropped off.

Letter 401

To W. D. Crick.

Down, Feb. 25th, 1882.

I am much obliged for your clear and distinct answers to  
my questions. I am sorry to trouble you, but there is one  
point which I do not fully understand. Did the shell remain  
attached to the beetle's leg from the 20th ...

off, both being in water, that the beetle's antenna was again temporarily caught by the shell?

I presume that I may keep the specimen till I go to London, which will be about the middle of next month.

I have placed the shell in fresh-water, to see if the valve will open, and whether it is still alive, for this seems to me a very interesting point. As the wretched beetle was still feebly alive, I have put it in a bottle with chopped laurel leaves, that it may die an easy and quicker death. I hope that I shall meet with your approval in doing so.

One of my sons tells me that on the coast of N. Wales the bare fishing hooks often bring up young mussels which have seized hold of the points; but I must make further enquiries on this head.

To W. D. Crick.

Letter

Down, March 23rd, 1882.

I have had a most unfortunate and extraordinary accident with your shell. I sent it by post in a strong box to Mr. Gwyn Jeffreys to be named, and heard two days afterwards that he had started for Italy. I then wrote to the servant in charge of his house to open the parcel (within which was a cover stamped and directed to myself) and return it to me. This servant, I suppose, opened the box and dropped the glass tube on a stone floor, and perhaps put his foot on it, for the tube and shell were broken into quite small fragments. These were returned to me with no explanation, the box being quite uninjured. I suppose you would not care for the fragments to be returned or the *Dytiscus*; but if you wish for them they shall be returned. I am very sorry, but it has not been my fault.

It seems to me almost useless to send the fragments of the shell to the British Museum to be named, more especially as the umbo has been lost. It is many years since I have looked at a fresh-water shell, but I should have said that the shell was *Cyclas cornea*.<sup>1</sup> Is *Sphaenium corneum* a synonym of *Cyclas*? Perhaps you could tell by looking to Mr. G. Jeffreys' book. If so, may we venture to call it so, or shall I put an (?) to the name?

As soon as I hear from you I will send my letter to *Nature*

## CHAPTER VIII

### MAN

I. *Descent of Man*.—II. *Sexual Selection*.—III. *Expression of the Emotions*

Letter 403

I. DESCENT OF MAN, 1860-82

To C. Lyell.

Down, April 27th [1860].

I cannot explain why, but to me it would be an infinite satisfaction to believe that mankind will progress to such a pitch that we should [look] back at [ourselves] as mere Barbarians. I have received proof-sheets (with a wonderfully nice letter) of very hostile review by Andrew Murray,<sup>1</sup> read before the Royal Society of Edinburgh. But I am tired with answering it. Indeed I have done nothing the whole day but answer letters.

Letter 404

To L. Horner.

The following letter occurs in the *Memoir of Leonard Horner*, edited by his daughter Katherine M. Lyell, Vol. II., p. 300 (privately printed, 1890).

Down, March 20th [1861].

I am very much obliged for your Address,<sup>2</sup> which has interested me much. . . . I thought that I had read up pretty well on the antiquity of man ; but you bring all the facts so well

<sup>1</sup> "On Mr. Darwin's Theory of the Origin of Species," by Andrew Murray. *Proc. Roy. Soc., Edinb.*, Vol. IV., pp. 274-91, 1862. The review concludes with the following sentence: "I have come to be of opinion that Mr. Darwin's theory is unsound, and that I am to be spared any collision between my inclination and my convictions" (referring to

together in a condensed focus, that the case seems much Lett clearer to me. How curious about the Bible!<sup>1</sup> I declare I had fancied that the date was somehow in the Bible. You are coming out in a new light as a Biblical critic. I must thank you for some remarks on the *Origin of Species*<sup>2</sup> (though I suppose it is almost as incorrect to do so as to thank a judge for a favourable verdict): what you have said has pleased me extremely. I am the more pleased, as I would rather have been well attacked than have been handled in the namby-pamby, old-woman style of the cautious Oxford Professor.<sup>3</sup>

To J. D. Hooker.

Lett

Mr. Wallace was, we believe, the first to treat the evolution of Man in any detail from the point of view of Natural Selection, namely, in a paper in the *Anthropological Review and Journal of the Anthropological Society*, May 1864, p. clviii. The deep interest with which Mr. Darwin read his copy is graphically recorded in the continuous series of pencil-marks along the margins of the pages. His views are fully given in Letter 406. The phrase, "in this case it is too far," refers to Mr. Wallace's habit of speaking of the theory of Natural Selection as due entirely to Darwin.

May 22nd 1864.

I have now read Wallace's paper on Man, and think it *most* striking and original and forcible. I wish he had written

<sup>1</sup> At p. lxxviii. Mr. Horner points out that the "chronology, given in the margin of our Bibles," i.e. the statement that the world was created 4004 B.C., is the work of Archbishop Usher, and is in no way binding on those who believe in the inspiration of Scripture. Mr. Horner goes on (p. lxx): "The retention of the marginal note in question is by no means a matter of indifference; it is untrue, and therefore it is mischievous." It is interesting that Archbishop Sumner and Dr. Dawes, Dean of Hereford, wrote with approbation of Mr. Horner's views on Man. The Archbishop says: "I have always considered the first verse of Genesis as indicating, rather than denying, a *preadamite* world" (*Memoir of Leonard Horner*, II., p. 303).

<sup>2</sup> Mr. Horner (p. xxxix) begins by disclaiming the qualifications of a competent critic, and confines himself to general remarks on the philosophic candour and freedom from dogmatism of the *Origin*: he does, however, give an opinion on the geological chapters IX. and X. As a general criticism he quotes Mr. Huxley's article in the *Westminster Review*, which may now be read in *Collected Essays*, II., p. 22.

<sup>3</sup> This no doubt refers to Professor Phillips' *Life on the Earth*, 1860,

Letter 405 Lyell's chapters on Man.<sup>1</sup> I quite agree about his high-mindedness, and have long thought so; but in this case it is too far, and I shall tell him so. I am not sure that I fully agree with his views about Man, but there is no doubt, in my opinion, on the remarkable genius shown by the paper. I agree, however, to the main new leading idea.

Letter 406

To A. R. Wallace.<sup>2</sup>

Down, [May] 28th [1864].

I am so much better that I have just finished a paper for the Linnean Society<sup>3</sup>; but I am not yet at all strong, I felt much disinclination to write, and therefore you must forgive me for not having sooner thanked you for your paper on Man<sup>4</sup> received on the 11th.<sup>5</sup> But first let me say that I have hardly ever in my life been more struck by any paper than that on "Variation," etc., etc., in the *Reader*.<sup>6</sup> I feel sure that such papers will do more for the spreading of our views on the modification of species than any separate treatises on the simple subject itself. It is really admirable; but you ought not in the Man paper to speak of the theory as mine; it is just as much yours as mine. One correspondent

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<sup>1</sup> See *Life and Letters*, III., p. 11 *et seq.* for Darwin's disappointment over Lyell's treatment of the evolutionary question in his *Antiquity of Man*; see also p. 29 for Lyell's almost pathetic words about his own position between the discarded faith of many years and the new one not yet assimilated. See also Letters 132, 164, 170.

<sup>2</sup> This letter was published in *Life and Letters*, III., p. 89.

<sup>3</sup> On the three forms, etc., of *Lythrum*.

<sup>4</sup> *Anthropological Review*, May 1864.

<sup>5</sup> Mr. Wallace wrote, May 10th, 1864: "I send you now my little contribution to the theory of the origin of man. I hope you will be able to agree with me. If you are able [to write] I shall be glad to have your criticisms. I was led to the subject by the necessity of explaining the vast mental and cranial differences between man and the apes combined with such small structural differences in other parts of the body,—and also by an endeavour to account for the diversity of human races combined with man's almost perfect stability of form during all historical epochs."

<sup>6</sup> *Reader*, April 1864.



has already noticed to me your "high-minded" conduct on this head.

But now for your Man paper, about which I should like to write more than I can. The great leading idea is quite new to me—viz. that during late ages the mind will have been modified more than the body; yet I had got as far as to see with you, that the struggle between the races of man depended entirely on intellectual and moral qualities. The latter part of the paper I can designate only as grand and most eloquently done. I have shown your paper to two or three persons who have been here, and they have been equally struck with it. I am not sure that I go with you on all minor points: when reading Sir G. Grey's account of the constant battles of Australian savages, I remember thinking that Natural Selection would come in, and likewise with the Esquimaux, with whom the art of fishing and managing canoes is said to be hereditary. I rather differ on the rank, under a classificatory point of view, which you assign to man; I do not think any character simply in excess ought ever to be used for the higher divisions. Ants would not be separated from other hymenopterous insects, however high the instinct of the one, and however low the instincts of the other. With respect to the differences of race, a conjecture has occurred to me that much may be due to the correlation of complexion (and consequently hair) with constitution. Assume that a dusky individual best escaped miasma, and you will readily see what I mean. I persuaded the Director-General of the Medical Department of the Army to send printed forms to the surgeons of all regiments in tropical countries to ascertain this point, but I daresay I shall never get any returns. Secondly, I suspect that a sort of sexual selection has been the most powerful means of changing the races of man. I can show that the different races have a widely different standard of beauty. Among savages the most powerful men will have the pick of the women, and they will generally leave the most descendants. I have collected a few notes on man, but I do not suppose I shall ever use them. Do you intend to follow out your views? and if so, would you like at some future time to have my few references

have not strength.

P.S. Our aristocracy is handsomer (more hideous according to a Chinese or Negro) than the middle classes, from [having the] pick of the women; but oh, what a scheme is primogeniture for destroying Natural Selection! I fear my letter will be barely intelligible to you.

Letter  
406\*

A. R. Wallace to C. Darwin.

5, Westbourne Grove Terrace, W.,  
May 29th [1864].

You are always so ready to appreciate what others do, and especially to overestimate my desultory efforts, that I cannot be surprised at your very kind and flattering remarks on my papers. I am glad, however, that you have made a few critical observations (and am only sorry that you were not well enough to make more), as that enables me to say a few words in explanation.

My great fault is haste. An idea strikes me, I think over it for a few days, and then write away with such illustrations as occur to me while going on. I therefore look at the subject almost solely from one point of view. Thus, in my paper on Man,<sup>1</sup> I aim solely at showing that brutes are modified in a great variety of ways by Natural Selection, but that in none of these particular ways can Man be modified, because of the superiority of his intellect. I therefore no doubt overlook a few smaller points in which Natural Selection may still act on men and brutes alike. Colour is one of them, and I have alluded to this in correlation to constitution, in an abstract I have made at Selater's request for the *Natural History Review*.<sup>2</sup> At the same time, there is so much evidence of migrations and displacements of races of man, and so many cases of peoples of distinct physical characters inhabiting the same or similar regions, and also of races of uniform physical characters inhabiting widely dissimilar regions,—that the external characteristics of the chief races of man must, I think, be older than his present geographical distribution, and the modifications produced by correlation to favourable variations of constitution be only a secondary

cause of external modification. I hope you may get the returns from the Army.<sup>1</sup> They would be very interesting, but I do not expect the results would be favourable to your view.

With regard to the constant battles of savages leading to selection of physical superiority, I think it would be very imperfect and subject to so many exceptions and irregularities that it could produce no definite result. For instance: the strongest and bravest men would lead, and expose themselves most, and would therefore be most subject to wounds and death. And the physical energy which led to any one tribe delighting in war, might lead to its extermination, by inducing quarrels with all surrounding tribes and leading them to combine against it. Again, superior cunning, stealth, and swiftness of foot, or even better weapons, would often lead to victory as well as mere physical strength. Moreover, this kind of more or less perpetual war goes on among all savage peoples. It could lead, therefore, to no differential characters, but merely to the keeping up of a certain average standard of bodily and mental health and vigour.

So with selection of variations adapted to special habits of life as fishing, paddling, riding, climbing, etc., etc., in different races, no doubt it must act to some extent, but will it be ever so rigid as to induce a definite physical modification, and can we imagine it to have had any part in producing the distinct races that now exist?

The sexual selection you allude to will also, I think, have been equally uncertain in its results. In the very lowest tribes there is rarely much polygamy, and women are more or less a matter of purchase. There is also little difference of social condition, and I think it rarely happens that any healthy and undeformed man remains without wife and children. I very much doubt the often-repeated assertion that our aristocracy are more beautiful than the middle classes. I allow that they present specimens of the highest kind of beauty, but I doubt the average. I have noticed in country places a greater average amount of good looks among the middle classes, and besides we unavoidably combine in

our idea of beauty, intellectual expression, and refinement manner, which often makes the less appear the more beautiful. Mere physical beauty—*i.e.* a healthy and regular development of the body and features approaching to the mean and type of European man, I believe is quite frequent in one class of society as the other, and much more frequent in rural districts than in cities.

With regard to the rank of man in zoological classification I fear I have not made myself intelligible. I never meant to adopt Owen's or any other such views, but only to point out that from one point of view he was right. I hold that a distinct family for Man, as Huxley allows, is all that can possibly be given him zoologically. But at the same time, my theory is true, that while the animals which surround him have been undergoing modification in all parts of the bodies to a generic or even family degree of difference, Man has been changing almost wholly in the brain and head—then in geological antiquity the *species* man may be as old as many mammalian families, and the origin of the *family* man may date back to a period when some of the *orders* first originated.

As to the theory of Natural Selection itself, I shall always maintain it to be actually yours and yours only. You have worked it out in details I had never thought of, years before I had a ray of light on the subject, and my paper would never have convinced anybody or been noticed as more than an ingenious speculation, whereas your book has revolutionised the study of Natural History, and carried away captive the best men of the present age. All the merit I claim is that having been the means of inducing you to write and publish at once. I may possibly some day go a little more into the subject (of Man), and if I do will accept the kind offer of your notes.

I am now, however, beginning to write the "Narrative of my Travels," which will occupy me a long time, as I have to write narrative, and after Bates' brilliant success rather fear to fail.

I shall introduce a few chapters on Geographical Distribution and other such topics. Sir C. Lyell, while agreeing with me

ciate the immense interval even to the later Pliocene. But I still maintain my view, which in fact is a logical result of my theory; for if man originated in later Pliocene, when almost all mammalia were of closely allied species to those now living, and many even identical, then man has not been stationary in bodily structure while animals have been varying, and my theory will be proved to be all wrong.

In Murchison's address to the Geographical Society, just delivered, he points out Africa as being the oldest existing land. He says there is no evidence of its having been ever submerged during the Tertiary epoch. Here then is evidently the place to find early man. I hope something good may be found in Borneo, and that the means may be found to explore the still more promising regions of tropical Africa, for we can expect nothing of man very early in Europe.

It has given me great pleasure to find that there are symptoms of improvement in your health. I hope you will not exert yourself too soon or write more than is quite agreeable to you. I think I made out every word of your letter, though it was not always easy.

For Wallace's later views see the small type on p. 39.

To W. Turner.

Sir William Turner is frequently referred to in the *Descent of Man* as having supplied Mr. Darwin with information.

Down, Dec. 14th [1866].

Your kindness when I met you at the Royal Society makes me think that you would grant me the favour of a little information, if in your power. I am preparing a book on Domestic Animals, and as there has been so much discussion on the bearing of such views as I hold on Man, I have some thoughts of adding a chapter on this subject. The point on which I want information is in regard to any part which may be fairly called rudimentary in comparison with the same part in the *Quadrumanus* or any other mammal. Now the os coccyx is rudimentary as a tail, and I am anxious to hear about its muscles. Mr. Flower found for me in some work that its one muscle (with *striæ*) was supposed only to bring

information.

Are there any traces of other muscles? It seems strange if there are none. Do you know how the muscles are in this part in the anthropoid apes? The muscles of the ear in man may, I suppose, in most cases be considered as rudimentary; and so they seem to be in the anthropoids at least, I am assured in the Zoological Gardens they do not erect their ears. I gather there are a good many muscles in various parts of the body which are in this same state: could you specify any of the best cases? The mammæ in man are rudimentary. Are there any other glands or other organs which you can think of? I know I have no right whatever to ask all these questions, and can only say that I should be grateful for any information. If you tell me anything about the os coccyx or other structures, I hope that you will permit me to quote the statement on your authority, as that would add so greatly to its value.

Pray excuse me for troubling you, and do not hurry yourself in the least in answering me.

I do not know whether you would care to possess a copy, but I told my publisher to send you a copy of the new edition of the *Origin* last month.

Letter 408

To W. Turner.

Down, Feb. 1st [1867].

I thank you cordially for all your full information, and I regret much that I have given you such great trouble at a period when your time is so much occupied. But the facts were so valuable to me that I cannot pretend that I am sorry that I did trouble you; and I am the less so, as from what you say I hope you may be induced some time to write a full account of all rudimentary structures in Man: it would be a very curious and interesting memoir. I shall at present give only a brief abstract of the chief facts which you have so very kindly communicated to me, and will not touch on some of the doubtful points. I have received far more information than I ventured to anticipate. There is one point which has occurred to me that I have not mentioned, and which I

is nothing in the notion. I have included the down on the human body and the lanugo on the foetus as a rudimentary representation of a hairy coat.<sup>1</sup> I do not know whether there is any direct functional connection between the presence of hair and the panniculus carnosus<sup>2</sup> (to put the question under another point of view, is it the primary or aboriginal function of the panniculus to move the dermal appendages or the skin itself?); but both are superficial, and would perhaps together become rudimentary. I was led to think of this by the places (as far as my ignorance of anatomy has allowed me to judge) of the rudimentary muscular fasciculi which you specify. Now, some persons can move the skin of their hairy heads; and is this not effected by the panniculus? How is it with the eyebrows? You specify the axillæ and the front region of the chest and lower part of scapulæ: now, these are all hairy spots in man. On the other hand, the neck, and as I suppose the covering of the gluteus medius, are not hairy; so, as I said, I presume there is nothing in this notion. If there were, the rudiments of the panniculus ought perhaps to occur more plainly in man than in woman. . . .

P.S.—If the skin on the head is moved by the panniculus, I think I ought just to allude to it, as some men alone having power to move the skin shows that the apparatus is generally rudimentary.

In March 1869 Darwin wrote to Mr. Wallace: "I shall be intensely curious to read the *Quarterly*. I hope you have not murdered too completely your own and my child." The reference is to Mr. Wallace's review, in the April number of the *Quarterly*, of Lyell's *Principles of Geology* (tenth edition), and of the sixth edition of the *Elements of Geology*. Mr. Wallace points out that here for the first time Sir C. Lyell gave up his opposition to evolution; and this leads Mr. Wallace to give a short account of the views set forth in the *Origin of Species*. In this article Mr. Wallace makes a definite statement as to his views on the evolution of man, which were opposed to those of Mr. Darwin. He upholds the view that the brain of man, as well as the organs of speech, the hand and the external form, could not have been evolved by Natural Selection (the child he is supposed to murder). At p. 391 he

Letter 408 writes: "In the brain of the lowest savages, and, as far as we know, of the prehistoric races, we have an organ . . . little inferior in size and complexity to that of the highest types. . . . But the mental requirements of the lowest savages, such as the Australians or the Andaman Islanders, are very little above those of many animals. . . . How, then, was an organ developed so far beyond the needs of its possessor? Natural Selection could only have endowed the savage with a brain a little superior to that of an ape, whereas he actually possesses one but very little inferior to that of the average members of our learned societies." This passage is marked in Mr. Darwin's copy with a triply underlined "No," and with a shower of notes of exclamation. It was probably the first occasion on which he realised the extent of this great and striking divergence in opinion between himself and his colleague.

He had, however, some indication of it in Wallace's paper on Man, *Anthropological Review*, 1864. (See Letter 406.) He wrote to Lyell, May 4th, 1869, "I was dreadfully disappointed about Man; it seems to me incredibly strange." And to Mr. Wallace, April 14th, 1869, "If you had not told me, I should have thought that [your remarks on Man] had been added by some one else. As you expected, I differ grievously from you, and I am very sorry for it."

Letter 409

To T. H. Huxley.

Down, Thursday, Feb. 21st [1868-70?].

I received the Jermyn Street programme, but have hardly yet considered it, for I was all day on the sofa on Tuesday and Wednesday. Bad though I was, I thought with constant pleasure of your very great kindness in offering to read the proofs of my essay on man. I do not know whether I said anything which might have appeared like a hint, but I assure you that such a thought had never even momentarily passed through my mind. Your offer has just made all the difference, that I can now write, whether or no my essay is ever printed, with a feeling of satisfaction instead of vague dread.

Beg my colleague, Mrs. Huxley, not to forget the corrugator supercilii: it will not be easy to catch the exact moment when the child is on the point of crying, and is struggling against the wrinkling up [of] its little eyes; for then I should expect the corrugator, from being little under the command of the will, would come into play in checking or stopping the wrinkling. An explosion of tears would tell nothing.



To Francis Galton.

Letter 410

Down, Dec. 23rd [1870?].

I have only read about fifty pages of your book (to the Judges),<sup>1</sup> but I must exhale myself, else something will go wrong in my inside. I do not think I ever in all my life read anything more interesting and original. And how well and clearly you put every point! George, who has finished the book, and who expressed himself just in the same terms, tells me the earlier chapters are nothing in interest to the later ones! It will take me some time to get to these later chapters, as it is read aloud to me by my wife, who is also much interested. You have made a convert of an opponent in one sense, for I have always maintained that, excepting fools, men did not differ much in intellect, only in zeal and hard work; and I still think [this] is an eminently important difference. I congratulate you on producing what I am convinced will prove a memorable work. I look forward with intense interest to each reading, but it sets me thinking so much that I find it very hard work; but that is wholly the fault of my brain, and not of your beautifully clear style.

To W. R. Greg.<sup>2</sup>

Letter 411

March 21st [1871?].

Many thanks for your note. I am very glad indeed to read remarks made by a man who possesses such varied and odd knowledge as you do, and who is so acute a reasoner. I have no doubt that you will detect blunders of many kinds in my book.<sup>3</sup> Your MS. on the proportion of the sexes at birth seems to me extremely curious, and I hope that some day you will publish it. It certainly appears that the males are decreasing in the London districts, and a most strange fact it is. Mr. Graham, however, I observe in a note enclosed, does not seem inclined to admit your conclusion. I have never

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<sup>1</sup> *Hereditary Genius: an Inquiry into its Laws and Consequences*, by Francis Galton, London, 1869. "The Judges of England between 1660 and 1865" is the heading of a section of this work (p. 55). See *Descent of Man* (1901), p. 41.

<sup>2</sup> Author of *The Enigmas of Life*, 1872.

<sup>3</sup> *The Descent of Man*.

Letter 411 much considered the subject of the causes of the proportion. When I reflected on queen bees producing only males when not impregnated, whilst some other parthenogenetic insects produced, as far as known, only females, the subject seemed to me hopelessly obscure. It is, however, pretty clear that you have taken the one path for its solution. I wished only to ascertain how far with various animals the males exceeded the females, and I have given all the facts which I could collect. As far as I know, no other data have been published. The equality of the sexes with race-horses is surprising. My remarks on mankind are quite superficial, and given merely as some sort of standard for comparison with the lower animals. M. Thury is the writer who makes the sex depend on the period of impregnation. His pamphlet<sup>1</sup> was sent me from Geneva. I can lend it you if you like. I subsequently read an account of experiments which convinced me that M. Thury was in error; but I cannot remember what they were, only the impression that I might safely banish this view from my mind. Your remarks on the less ratio of males in illegitimate births strikes me as the most doubtful point in your MS.—requiring two assumptions, viz. that the fathers in such cases are relatively too young, and that the result is the same as when the father is relatively too old.

My son George, who is a mathematician, and who read your MS. with much interest, has suggested, as telling in the right direction, but whether sufficient is another question, that many more illegitimate children are murdered and concealed shortly after birth, than in the case of legitimate children; and as many more males than females die during the first few days of life, the census of illegitimate children practically applies to an older age than with legitimate children, and would thus slightly reduce the excess of males. This might possibly be worth consideration. By a strange coincidence a stranger writes to me this day, making the very same suggestion.

I am quite delighted to hear that my book interests you enough to lead you to read it with some care.

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<sup>1</sup> *Mémoire sur la loi de Production des Sexes*, 2nd edit., 1863 (a pamphlet published by Cherbuliez, Geneva).

To Francis Galton.

Letter 412

Down, Jan. 4th, 1873.

Very many thanks for *Fraser*:<sup>1</sup> I have been greatly interested by your article. The idea of castes being spontaneously formed and leading to intermarriage<sup>2</sup> is quite new to me, and I should suppose to others. I am not, however, so hopeful as you. Your proposed Society<sup>3</sup> would have awfully laborious work, and I doubt whether you could ever get efficient workers. As it is, there is much concealment of insanity and wickedness in families; and there would be more if there was a register. But the greatest difficulty, I think, would be in deciding who deserved to be on the register. How few are above mediocrity in health, strength, morals and intellect; and how difficult to judge on these latter heads. As far as I see, within the same large superior family, only a few of the children would deserve to be on the register; and these would naturally stick to their own families, so that the superior children of distinct families would have no good chance of associating much, forming a caste. Though I see so much difficulty, the obj seems a grand one; and you have pointed out the some feasible, yet I fear utopian, plan of procedure in improving the human race. I should be inclined to trust more (and this is part of your plan) to disseminating and insisting on the importance of the all-important principle of inheritance. I will make one or two minor criticisms. Is it not possible that

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<sup>1</sup> "Hereditary Improvement," by Francis Galton, *Fraser's Magazine*, Jan. 1873, p. 116.

<sup>2</sup> "My object is to build up, by the mere process of extensive enquiry and publication of results, a sentiment of caste among those who are naturally gifted, and to procure for them, before the system has fairly taken root, such moderate social favours and preference, no more no less, as would seem reasonable to those who were justly informed of the precise measure of their importance to the nation" (*loc. cit.*, p. 123).

<sup>3</sup> Mr. Galton proposes that "Some society should undertake three scientific services: the first, by means of a moderate number of influential local agencies, to institute continuous enquiries into the facts of human heredity; the second to be a centre of information on heredity for breeders of animals and plants; and the third to discuss and classify the facts that were collected" (*loc. cit.*, p. 124).

412 the inhabitants of malarious countries owe their degraded and miserable appearance to the bad atmosphere, though this does not kill them, rather than to "economy of structure"? I do not see that an orthognathous face would cost more than a prognathous face; or a good morale than a bad one. That is a fine simile (p. 119) about the chip of a statue;<sup>1</sup> but surely Nature does not more carefully regard races than individuals, as (I believe I have misunderstood what you mean) evidenced by the multitude of races and species which have become extinct. Would it not be truer to say that Nature cares only for the superior individuals and then makes her new and better races? But we ought both to shudder in using so freely the word "Nature"<sup>2</sup> after what De Candolle has said. Again let me thank you for the interest received in reading your essay.

Many thanks about the rabbits; your letter has been sent to Balfour:<sup>3</sup> he is a very clever young man, and I believe owes his cleverness to Salisbury blood. This letter will not be worth your deciphering. I have almost finished Greg's *Enigmas*.<sup>4</sup> It is grand poetry—but too Utopian and too full of faith for me; so that I have been rather disappointed. What do you think about it? He must be a delightful man.

I doubt whether you have made clear how the families on the Register are to be kept pure or superior, and how they are to be in course of time still further improved.

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<sup>1</sup> "... The life of the individual is treated as of absolutely no importance, while the race is as everything; Nature being wholly careless of the former except as a contributor to the maintenance and evolution of the latter. Myriads of inchoate lives are produced in what, to our best judgment, seems a wasteful and reckless manner, in order that a few selected specimens may survive, and be the parents of the next generation. It is as though individual lives were of no more consideration than are the senseless chips which fall from the chisel of the artist who is elaborating some ideal form from a rude block" (*loc. cit.*, p. 119).

<sup>2</sup> See Letter 190, Vol. I., p. 269.

<sup>3</sup> Francis Maitland Balfour (1851-82) was Professor of Animal Morphology at Cambridge. (See *Life and Letters*, III., p. 250.)

<sup>4</sup> *The Enigmas of Life*, 1872.

## To Max Müller.

Letter 413

Down, July 3rd, 1873.

In June, 1873, Professor Max Müller sent to Mr. Darwin a copy of the sixth edition of his *Lectures on the Science of Language*,<sup>1</sup> with a letter concluding with these words: "I venture to send you my three lectures, trusting that, though I differ from some of your conclusions, you will believe me to be one of your diligent readers and sincere admirers."

I am much obliged for your kind note and present of your lectures. I am extremely glad to have received them from you, and I had intended ordering them.

I feel quite sure from what I have read in your works that you would never say anything of an honest adversary to which he would have any just right to object; and as for myself, you have often spoken highly of me—perhaps more highly than I deserve.

As far as language is concerned I am not worthy to be your adversary, as I know extremely little about it, and that little learnt from very few books. I should have been glad to have avoided the whole subject, but was compelled to take it up as well as I could. He who is fully convinced, as<sup>1</sup> --- that man is descended from some lower animal, is forced to believe *a priori* that articulate language has developed from inarticulate cries<sup>2</sup>; and he is therefore hardly a fair judge of the arguments opposed to this belief.

In October, 1875, Mr. Darwin again wrote cordially to Professor Max Müller on receipt of a pamphlet entitled *In Self-Defence*,<sup>3</sup> which is a reply to Professor Whitney's "Darwinism and Language" in the *North American Review*, July, 1874. This essay had been brought before the "general reader" in England by an article of Mr. G. Darwin's in the *Contemporary Review*, November, 1874, p. 894, entitled, "Professor Whitney on the Origin of Language." The article was followed by "My Reply to Mr. Darwin," contributed by Professor Müller to the *Contemporary Review*, January, 1875, p. 305.

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<sup>1</sup> A reference to the first edition occurs in *Life and Letters*, II., p. 390.

<sup>2</sup> *Descent of Man* (1901), p. 133.

<sup>3</sup> Printed in *Chips from a German Workshop*, Vol. IV., 1875, p. 473.

Letter 414

G. Rolleston<sup>1</sup> to C. Darwin.

British Association, Bristol, August 30th, 1875.

In the first edition of the *Descent of Man* Mr. Darwin wrote: "It is a more curious fact that savages did not formerly waste away, as Mr. Bagehot has remarked, before the classical nations, as they now do before modern civilised nations. . . ."<sup>2</sup> In the second edition (p. 183) the statement remains, but a mass of evidence (pp. 183-92) is added, to which reference occurs in the reply to the following letter.

At pp. 4-5 of the enclosed Address<sup>3</sup> you will find that I have controverted Mr. Bagehot's view as to the extinction of the barbarians in the times of classical antiquity, as also the view of Pöppig as to there being some occult influence exercised by civilisation to the disadvantage of savagery when the two come into contact.

I write to say that I took up this subject without any wish to impugn any views of yours as such, but with the desire of having my say upon certain anti-sanitarian transactions and malfeasance of which I had had a painful experience.

On reading however what I said, and had written somewhat hastily, it has struck me that what I have said might bear the former interpretation in the eyes of persons who might not read other papers of mine, and indeed other parts of the same Address, in which my adhesion, whatever it is worth, to your views in general is plainly enough implied. I have ventured to write this explanation to you for several reasons.

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<sup>1</sup> George Rolleston (1829-81) obtained a first-class in Classics at Oxford in 1850; he was elected Fellow of Pembroke College in 1851, and in the same year he entered St. Bartholomew's Hospital. Towards the close of the Crimean War, Rolleston was appointed one of the Physicians to the British civil hospital at Smyrna. In 1860 he was elected the first Linacre Professor of Anatomy and Physiology, a post which he held until his death. "He was perhaps the last of a school of English natural historians or biologists in the widest sense of the term." In 1862 he gave the results of his work on the classification of brains in a lecture delivered at the Royal Institution, and in 1870 published his best known book, *Forms of Animal Life* (*Dict. Nat. Biography*).

<sup>2</sup> Bagehot, "Physics and Politics," *Fortnightly Review*, April, 1868, p. 455.

<sup>3</sup> *British Association Reports*, 1875, p. 142.

To G. Rolleston.

Letter 45.

Bassett, Southampton, Sept. 2nd [1875].

I am much obliged to you for having sent me your Address, which has interested me greatly. I quite subscribe to what you say about Mr. Bagehot's striking remark, and wish I had not quoted it. I can perceive no sort of reflection or blame on anything which I have written, and I know well that I deserve many a good slap on the face. The decrease of savage populations interests me much, and I should like you some time to look at a discussion on this subject which I have introduced in the second edition of the *Descent of Man*, and which you can find (for I have no copy here) in the list of additions. The facts have convinced me that lessened fertility and the poor constitution of the children is one chief cause of such decrease; and that the case is strictly parallel to the sterility of many wild animals when made captive, the civilisation of savages and the captivity of wild animals leading to the same result.

To Ernst Krause.

Letter 416

Down, June 30th, 1877.

I have been much interested by your able argument<sup>1</sup> against the belief that the sense of colour has been recently acquired by man. The following observation bears on this subject.

I attended carefully to the mental development of my young children, and with two, or as I believe three of them, soon after they had come to the age when they knew the names of all common objects, I was startled by observing that they seemed quite incapable of affixing the right names to the colours in coloured engravings, although I tried repeatedly to teach them. I distinctly remember declaring that they were colour-blind, but this afterwards proved a groundless fear.

On communicating this fact to another person he told me that he had observed a nearly similar case. Therefore the difficulty which young children experience either in distinguishing, or more probably in naming colours, seems to

<sup>1</sup> See *Kosmos*, June 1877, p. 264, a review of Dr. Hugo Magnus' *Die Geschichtliche Entwicklung des Farbensinnes*, 1877. The first part is chiefly an account of the author's views; Dr. Krause's argument begins at p. 269. The interest felt by Mr. Darwin is recorded by the numerous pencil-marks on the margin of his copy.

Letter 416 deserve further investigation. I will add that it formerly appeared to me that the gustatory sense, at least in the case of my own infants, and very young children, differed from that of grown-up persons. This was shown by their not disliking rhubarb mixed with a little sugar and milk, which is to us abominably nauseous; and in their strong taste for the sourest and most austere fruits, such as unripe gooseberries and crab apples.

Letter 417

To G. J. Romanes.

[Barlaston], Aug. 20th, 1878.

Part of this letter (here omitted) is published in *Life and Letters*, III., p. 225, and the whole in the *Life and Letters of G. J. Romanes*, p. 74. The lecture referred to was on animal intelligence, and was given at the Dublin meeting of the British Association.

. . . The sole fault which I find with your lecture is that it is too short, and this is a rare fault. It strikes me as admirably clear and interesting. I meant to have remonstrated that you had not discussed sufficiently the necessity of signs for the formation of abstract ideas of any complexity, and then I came on the discussion on deaf mutes. This latter seems to me one of the richest of all the mines, and is worth working carefully for years, and very deeply. I should like to read whole chapters on this one head, and others on the minds of the higher idiots. Nothing can be better, as it seems to me, than your several lines or sources of evidence, and the manner in which you have arranged the whole subject. Your book will assuredly be worth years of hard labour; and stick to your subject. By the way, I was pleased at your discussing the selection of varying instincts or mental tendencies; for I have often been disappointed by no one having ever noticed this notion.

I have just finished *La Psychologie, son Présent et son Avenir*, 1876, by Delbœuf (a mathematician and physicist of Belgium) in about a hundred pages. It has interested me a good deal, but why I hardly know; it is rather like Herber Spencer. If you do not know it, and would care to see it send me a postcard.

Thank Heaven, we return home on Thursday, and I shall be able to go on with my humdrum work, and that make me forget my daily discomfort.



Elliot & Fry photo.

Walker & Rockwell, ph. sc.

G. J. Romanes.  
1891.

Have you ever thought of keeping a young monkey, so as to observe its mind? At a house where we have been staying there were Sir A. and Lady Hobhouse, not long ago returned from India, and she and he kept a young monkey and told me some curious particulars. One was that her monkey was very fond of looking through her eyeglass at objects, and moved the glass nearer and further so as to vary the focus. This struck me, as Frank's son, nearly two years old (and we think much of his intellect!) is very fond of looking through my pocket lens, and I have quite in vain endeavoured to teach him not to put the glass close down on the object, but he always will do so. Therefore I conclude that a child under two years is inferior in intellect to a monkey. Once again I heartily congratulate you on your well-earned present, and I feel assured, grand future success.

Later in the year Mr. Darwin wrote: "I am delighted to hear that you mean to work the comparative Psychology well. I thought your letter to the *Times*<sup>1</sup> very good indeed. Bartlett, at the Zoological Gardens, I feel sure, would advise you infinitely better about hardiness, intellect, price, etc., of monkey than F. Buckland; but with him it must be *viva voce*."

"Frank says you ought to keep an idiot, a deaf mute, a monkey, and a baby in your house."

To

This letter has been published in Clapperton's *Scientific Meliorism*, 1885, p. 340, together with Mr. Gaskell's letter of Nov. 13th (p. 337). Mr. Gaskell's laws are given in his letter of Nov. 13th, 1878. They are:—

- I. The Organological Law:  
Natural Selection, or the Survival of the Fittest.
- II. The Sociological Law:  
Sympathetic Selection, or Indiscriminate Survival.
- III. The Moral Law:  
Social Selection, or the Birth of the Fittest.

Your letter seems to me very interesting and clearly expressed, and I hope that you are in the right. Your

<sup>1</sup> Romanes wrote to the *Times* August 28th, 1878, expressing his views regarding the distinction between man and the lower animals, in reply to criticisms contained in a leading article in the *Times* of August 23rd on his lecture at the Dublin meeting of the British Association.

Letter 418 second law appears to be largely acted on in all civilised countries, and I just alluded to it in my remarks to the effect (as far as I remember) that the evil which would follow by checking benevolence and sympathy in not fostering the weak and diseased would be greater than by allowing them to survive and then to procreate.

With regard to your third law, I do not know whether you have read an article (I forget when published) by F. Galton, in which he proposes certificates of health, etc., for marriage, and that the best should be matched. I have lately been led to reflect a little (for, now that I am growing old, my work has become [word indecipherable] special) on the artificial checks, but doubt greatly whether such would be advantageous to the world at large at present, however it may be in the distant future. Suppose that such checks had been in action during the last two or three centuries, or even for a shorter time in Britain, what a difference it would have made in the world, when we consider America, Australia, New Zealand, and S. Africa! No words can exaggerate the importance, in my opinion, of our colonisation for the future history of the world.

If it were universally known that the birth of children could be prevented, and this were not thought immoral by married persons, would there not be great danger of extreme profligacy amongst unmarried women, and might we not become like the "arerei" societies in the Pacific? In the course of a century France will tell us the result in many ways, and we can already see that the French nation does not spread or increase much.

I am glad that you intend to continue your investigations, and I hope ultimately may publish on the subject.

Letter 419

To K. Höchberg.

Down, Jan. 13th, 1879.

I am much obliged for your note and for the essay which you have sent me. I am a poor german scholar, and your german is difficult; but I think that I understand your meaning, and hope at some future time, when more at leisure, to recur to your essay. As far as I can judge, you have made a great advance in many ways in the subject; and I will

send your paper to Mr. Edmund Gurney,<sup>1</sup> who has written Letter 419 on and is much interested in the origin of the taste for music. In reading your essay, it occurred to me that facility in the utterance of prolonged sounds (I do not think that you allude to this point) may possibly come into play in rendering them musical; for I have heard it stated that those who vary their voices much, and use cadences in long continued speaking, feel less fatigued than those who speak on the same note.

To G. J. Romanes.

Letter 420

Down, Feb. 5th, 1880.

Romanes was at work on what ultimately came to be a book on animal intelligence. Romanes's reply to this letter is given in his *Life*, p. 95. The table referred to is published as a frontispiece to his *Mental Evolution in Animals*, 1885.

As I feared, I cannot be of the least use to you. I could not venture to say anything about babies without reading my Expression book and paper on Infants, or about animals without reading the *Descent of Man* and referring to my notes; and it is a great wrench to my mind to change from one subject to another.

I will, however, hazard one or two remarks. Firstly, I should have thought that the word "love" (not sexual passion), as shown very low in the scale, to offspring and apparently to comrades, ought to have come in more prominently in your table than appears to be the case. Secondly, if you give any instance of the appreciation of different stimulants by plants, there is a much better case than that given by you—namely, that of the glands of *Drosera*, which can be touched roughly two or three times and do not transmit any effect, but do so if pressed by a weight of  $\frac{1}{75000}$  grain (*Insectiv. Plants* 263). On the other hand, the filament of *Dionaea* may be quietly loaded with a much greater weight, while a touch by a hair causes the lobes to close instantly. This has always seemed to me a marvellous fact. Thirdly, I have been accustomed to look at the coming in of the sense of pleasure and pain as one of the most important steps in the development of mind, and I should think it ought to be prominent in your table. The sort of progress which I have imagined is that a stimulus produced some effect at the point affected, and

<sup>1</sup> The late Edmund Gurney, author of *The Power of Sound*, 1880.

Letter 420 that the effect radiated at first in all directions, and then that certain definite advantageous lines of transmission were acquired, inducing definite reaction in certain lines. Such transmission afterwards became associated in some unknown way with pleasure or pain. These sensations led at first to all sorts of violent action, such as the wriggling of a worm, which was of some use. All the organs of sense would be at the same time excited. Afterwards definite lines of action would be found to be the most useful, and so would be practised. But it is of no use my giving you my crude notions.

Letter 421

To S. Tolver Preston.

Down, May 22nd, 1880.

Your letter<sup>1</sup> appears to me an interesting and valuable one; but I have now been working for some years exclusively on the physiology of plants, and all other subjects have gone out of my head, and it fatigues me much to try and bring them back again into my head. I am, moreover, at present very busy, as I leave home for a fortnight's rest at the beginning of next week. My conviction as yet remains unchanged, that a man who (for instance) jumps into a river to save a life without a second's reflection (either from an innate tendency or from one gained by habit) is deservedly more honoured than a man who acts deliberately and is conscious, for however short a time, that the risk and sacrifice give him some inward satisfaction.

You are of course familiar with Herbert Spencer's writings on Ethics.

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<sup>1</sup> Mr. Preston wrote (May 20th, 1880) to the effect that "self-interest as a motive for conduct is a thing to be commended—and it certainly [is] I think . . . the only conceivable rational motive of conduct: and always is the tacitly recognised motive in all rational actions." Mr. Preston does not, of course, commend selfishness, which is not true self-interest.

There seem to be two ways of looking at the case given by Darwin. The man who knows that he is risking his life,—realising that the personal satisfaction that may follow is not worth the risk—is surely admirable from the strength of character that leads him to follow the social instinct against his purely personal inclination. But the man who blindly obeys the social instinct is a more useful member of a social community. He will act with courage where even the strong man will fail.

The observations to which the following letters refer were continued by Mr. Wallis, who gave an account of his work in an interesting paper in the *Proceedings of the Zoological Society*, March 2nd, 1897. The results on the whole confirm the belief that traces of an ancestral pointed ear exist in man.

To H. M. Wallis.

Letter 422

Down, March 22nd, 1881.

I am very much obliged for your courteous and kind note. The fact which you communicate is quite new to me, and as I was laughed at about the tips to human ears, I should like to publish in *Nature* some time your fact. But I must first consult Eschricht, and see whether he notices this fact in his curious paper on the lanugo on human embryos; and secondly I ought to look to monkeys and other animals which have tufted ears, and observe how the hair grows. This I shall not be able to do for some months, as I shall not be in London until the autumn so as to go to the Zoological Gardens. But in order that I may not hereafter throw away time, will you be so kind as to inform me whether I may publish your observation if on further search it seems desirable?

To H. M. Wallis.

Letter 423

Down, March 31st, 1881.

I am much obliged for your interesting letter. I am glad to hear that you are looking to other ears, and will visit the Zoological Gardens. Under these circumstances it would be incomparably better (as more authentic) if you would publish a notice of your observations in *Nature* or some scientific journal. Would it not be well to confine your attention to infants, as more likely to retain any primordial character, and offering less difficulty in observing. I think, though, it would be worth while to observe whether there is any relation (though probably none) between much hairiness on the ears of an infant and the presence of the "tip" on the folded

Letter 423 margin. Could you not get an accurate sketch of the direction of the hair of the tip of an ear?

The fact which you communicate about the goat-sucker is very curious. About the difference in the power of flight in Dorkings, etc., may it not be due merely to greater weight of body in the adults?

I am so old that I am not likely ever again to write on general and difficult points in the theory of Evolution.

I shall use what little strength is left me for more confined and easy subjects.

Letter 424

To Mrs. Talbot.

Mrs. Emily Talbot was secretary of the Education Department of the American Social Science Association, Boston, Mass. A circular and register was issued by the Department, and answers to various questions were asked for. See *Nature*, April 28th, p. 617, 1881. The above letter was published in *The Field Naturalist*, Manchester, 1883, p. 5, edited by Mr. W. E. Axon, to whom we are indebted for a copy.

Down, July 19th [1881?]

In response to your wish, I have much pleasure in expressing the interest which I feel in your proposed investigation on the mental and bodily development of infants. Very little is at present accurately known on this subject, and I believe that isolated observations will add but little to our knowledge, whereas tabulated results from a very large number of observations, systematically made, would probably throw much light on the sequence and period of development of the several faculties. This knowledge would probably give a foundation for some improvement in our education of young children, and would show us whether the system ought to be followed in all cases.

I will venture to specify a few points of inquiry which, as it seems to me, possess some scientific interest. For instance, does the education of the parents influence the mental powers of their children at any age, either at a very early or somewhat more advanced stage? This could perhaps be learned by schoolmasters and mistresses if a large number of children were first classed according to age and their mental attainments, and afterwards in accordance with the education of their parents, as far as this could be discovered. As observation

is one of the earliest faculties developed in young children, Letter 424 and as this power would probably be exercised in an equal degree by the children of educated and uneducated persons, it seems not impossible that any transmitted effect from education could be displayed only at a somewhat advanced age. It would be desirable to test statistically, in a similar manner, the truth of the oft-repeated statement that coloured children at first learn as quickly as white children, but that they afterwards fall off in progress. If it could be proved that education acts not only on the individual, but, by transmission, on the race, this would be a great encouragement to all working on this all-important subject. It is well known that children sometimes exhibit, at a very early age, strong special tastes, for which no cause can be assigned, although occasionally they may be accounted for by reversion to the taste or occupation of some progenitor; and it would be interesting to learn how far such early tastes are persistent and influence the future career of the individual. In some instances such tastes die away without apparently leaving any after effect, but it would be desirable to know how far this is commonly the case, as we should then know whether it were important to direct as far as this is possible the early tastes of our children. It may be more beneficial that a child should follow energetically some pursuit, of however trifling a nature, and thus acquire perseverance, than that he should be turned from it because of no future advantage to him. I will mention one other small point of inquiry in relation to very young children, which may possibly prove important with respect to the origin of language; but it could be investigated only by persons possessing an accurate musical ear. Children, even before they can articulate, express some of their feelings and desires by noises uttered in different notes. For instance, they make an interrogative noise, and others of assent and dissent, in different tones; and it would, I think, be worth while to ascertain whether there is any uniformity in different children in the pitch of their voices under various frames of mind.

I fear that this letter can be of no use to you, but it will serve to show my sympathy and good wishes in your researches.



## II. SEXUAL SELECTION, 1866-72.

Letter 425

To James Shaw.

Down, Feb. 11th [1866].

I am much obliged to you for your kindness in sending me an abstract of your paper<sup>1</sup> on beauty. In my opinion you take quite a correct view of the subject. It is clear that Dr. Dickson has either never seen my book, or overlooked the discussion on sexual selection. If you have any precise facts on birds' "courtesy towards their own image in mirror or picture," I should very much like to hear them. Butterflies offer an excellent instance of beauty being displayed in conspicuous parts; for those kinds which habitually display the underside of the wing have this side gaudily coloured, and this is not so in the reverse case. I daresay you will know that the males of many foreign butterflies are much more brilliantly coloured than the females, as in the case of birds. I can adduce good evidence from two large classes of facts (too large to specify) that flowers have become beautiful to make them conspicuous to insects.<sup>2</sup>

Mr. Darwin wrote again to Mr. Shaw in April, 1866 :—

I am much obliged for your kind letter and all the great trouble which you have taken in sending on all the various and interesting facts on birds admiring themselves. I am very glad to hear of these facts. I have just finished writing and adding to a new edition of the *Origin*, and in this have given, without going into details (so that I shall not be able to use your facts), some remarks on the subject of beauty.

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<sup>1</sup> A newspaper report of a communication to the *Dumfries Antiquarian and Natural History Society*.

<sup>2</sup> This letter is published in *A Country Schoolmaster*, James Shaw. Edited by Robert Wallace, Edinburgh, 1899.

To A. D. Bartlett.<sup>1</sup>

Letter 426

Down, Feb. 16th, [1867?]

I want to beg two favours of you. I wish to ascertain whether the Bower-Bird discriminates colours.<sup>2</sup> Will you have all the coloured worsted removed from the cage and bower, and then put all in a row, at some distance from bower, the enclosed coloured worsted, and mark whether the bird *at first* makes any selection. Each packet contains an equal quantity; the packets had better be separate, and each thread put separate, but close together; perhaps it would be fairest if the several colours were put alternately—one thread of bright scarlet, one thread of brown, etc., etc. There are six colours. Will you have the kindness to tell me whether the birds prefer one colour to another?

Secondly, I very much want several heads of the fancy and long-domesticated rabbits, to measure the capacity of skull. I want only small kinds, such as Himalaya, small Angora, Silver Grey, or any small-sized rabbit which has long been domesticated. The Silver Grey from warrens would be of little use. The animals must be adult, and the smaller the breed the better. Now when any one dies would you send me the carcase named; if the skin is of any value it might be skinned, but it would be rather better with skin, and I could make a present to any keeper to whom the skin is a perquisite. This would be of great assistance to me, if you would have the kindness thus to aid me.

To W. B. Tegetmeier.

Letter 427

We are not aware that the experiment here suggested has ever been carried out.

Down, March 5th [1867].

I write on the bare and very improbable chance of your being able to try, or get some trustworthy person to try,

<sup>1</sup> Abraham Dee Bartlett (1812-97) was resident superintendent of the Zoological Society's Gardens in Regent's Park from 1859 to 1897. He communicated several papers to the Zoological Society. His knowledge was always at the service of Mr. Darwin, who had a sincere respect for him.

<sup>2</sup> Mr. Bartlett does not seem to have supplied any information on the point in question. The evidence for the Bower-Bird's taste in colour is in *Descent of Man*, II., p. 112.

Letter 427 the following little experiment. But I may first state, as showing what I want, that it has been stated that if two long feathers in the tail of the male Widow-Bird at the Cape of Good Hope are pulled out, no female will pair with him.

Now, where two or three common cocks are kept; I want to know, if the tail sickle-feathers and saddle-feathers of one which had succeeded in getting wives were cut and mutilated and his beauty spoiled, whether he would continue to be successful in getting wives. This might be tried with drakes or peacocks, but no one would be willing to spoil for a season his peacocks. I have no strength or opportunity of watching my own poultry, otherwise I would try it. I would very gladly repay all expenses of loss of value of the poultry, etc. But, as I said, I have written on the most improbable chance of your interesting any one to make the trial, or having time and inclination yourself to make it. Another, and perhaps better, mode of making the trial would be to turn down to some hens two or three cocks, one being injured in its plumage.

I am glad to say that I have begun correcting proofs.<sup>1</sup> I hope that you received safely the skulls which you so kindly lent me.

Letter 428

To W. B. Tegetmeier.

Down, March 30th [1867].

I am much obliged for your note, and shall be truly obliged if you will insert any question on the subject. That is a capital remark of yours about the trimmed game cocks, and shall be quoted by me.<sup>2</sup> Nevertheless I am still inclined from many facts strongly to believe that the beauty of the male bird determines the choice of the female with wild birds, however it may be under domestication. Sir R. Heron has described how one pied peacock was extra attentive to the hens. This is a subject which I must take up as soon as my present book is done.

I shall be most particularly obliged to you if you will

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<sup>1</sup> *The Variation of Animals and Plants.*

<sup>2</sup> *Descent of Man*, Ed. 1., Vol. II., p. 117. "Mr. Tegetmeier is convinced that a game cock, though disfigured by being dubbed with his hackles trimmed, would be accepted as readily as a male retaining all his natural ornaments."

dye with magenta a pigeon or two.<sup>1</sup> Would it not be better, Letter 428  
to dye the tail alone and crown of head, so as not to make  
too great difference? I shall be very curious to hear how  
an entirely crimson pigeon will be received by the others  
as well as his mate.

P.S.—Perhaps the best experiment, for my purpose, would  
be to colour a young unpaired male and turn him with other  
pigeons, and observe whether he was longer or quicker than  
usual in mating.

To A. R. Wallace.

Letter 429

Down, April 29th [1867].

I have been greatly interested by your letter,<sup>2</sup> but your  
view is not new to me. If you will look at p. 240 of the  
fourth edition of the *Origin* you will find it very briefly given  
with two extreme examples of the peacock and black grouse.  
A more general statement is given at p. 101, or at p. 89  
of the first edition, for I have long entertained this view,  
though I have never had space to develop it. But I had  
not sufficient knowledge to generalise as far as you do  
about colouring and nesting. In your paper perhaps you  
will just allude to my scanty remark in the fourth edition,  
because in my Essay on Man I intend to discuss the whole  
subject of sexual selection, explaining as I believe it does  
much with respect to man. I have collected all my old  
notes, and partly written my discussion, and it would be  
flat work for me to give the leading idea as exclusively

<sup>1</sup> "Mr. Tegetmeier, at my request, stained some of his birds with  
magenta, but they were not much noticed by the others."—*Descent of  
Man* (1901), p. 637.

<sup>2</sup> We have not been able to find Mr. Wallace's letter to which this  
is a reply. It evidently refers to Mr. Wallace's belief in the paramount  
importance of protection in the evolution of colour. This is clear from  
the P.S. to the present letter and from the passages in the *Origin*  
referred to. The first reference, Ed. IV., p. 240, is as follows:  
"We can sometimes plainly see the proximate cause of the trans-  
mission of ornaments to the males alone; for a pea-hen with the  
long tail of the male bird would be badly fitted to sit on her eggs,  
and a coal-black female capercailzie would be far more conspicuous on  
her nest, and more exposed to danger, than in her present modest  
attire." The passages in Ed. I. (pp. 89, 101) do not directly bear on  
the question of protection.

Letter 429 from you. But, as I am sure from your greater knowledge of Ornithology and Entomology that you will write a much better discussion than I could, your paper will be of great use to me. Nevertheless I must discuss the subject fully in my Essay on Man. When we met at the Zoological Society, and I asked you about the sexual differences in kingfishers, I had this subject in view; as I had when I suggested to Bates the difficulty about gaudy caterpillars, which you have so admirably (as I believe it will prove) explained.<sup>1</sup> I have got one capital case (genus forgotten) of a [Australian] bird in which the female has long tail-plumes, and which consequently builds a different nest from all her allies.<sup>2</sup> With respect to certain female birds being more brightly coloured than the males, and the latter incubating, I have gone a little into the subject, and cannot say that I am fully satisfied. I remember mentioning to you the case of *Rhynchœa*, but its nesting seems unknown. In some other cases the difference in brightness seemed to me hardly sufficiently accounted for by the principle of protection. At the Falkland Islands there is a carrion hawk in which the female (as I ascertained by dissection) is the brightest coloured, and I doubt whether protection will here apply; but I wrote several months ago to the Falklands to make enquiries. The conclusion to which I have been leaning is that in some of these abnormal cases the colour happened to vary in the female alone, and was transmitted to females alone, and that her variations have been selected through the admiration of the male.

It is a very interesting subject, but I shall not be able to go on with it for the next five or six months, as I am fully employed in correcting dull proof-sheets. When I return to the work I shall find it much better done by you than I could have succeeded in doing.

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<sup>1</sup> See a letter of Feb. 26th, 1867, to Mr. Wallace, *Life and Letters* III. p. 94.

<sup>2</sup> *Menura superba*: see *Descent of Man* (1901), p. 687. *Rhynchœa* mentioned a line or two lower down, is discussed in the *Descent* p. 727. The female is more brightly coloured than the male, and has a convoluted trachea, elsewhere a masculine character. There seems some reason to suppose that "the male undertakes the duty of incubation."

It is curious how we hit on the same ideas. I have Letter 429  
endeavoured to show in my MS. discussion that nearly the  
same principles account for young birds not being gaily  
coloured in many cases, but this is too complex a point  
for a note.

On reading over your letter again, and on further  
reflection, I do not think (as far as I remember my words)  
that I expressed myself nearly strongly enough on the  
value and beauty of your generalisation,<sup>1</sup> viz., that all birds  
in which the female is conspicuously or brightly coloured  
build in holes or under domes. I thought that this was  
the explanation in many, perhaps most cases, but do  
not think I should ever have extended my view to your  
generalisation. Forgive me troubling you with this P.S.

To A. R. Wallace.

Letter 430

Down, May 5th [1867].

The offer of your valuable notes is most generous, but it  
would vex me to take so much from you, as it is certain that  
you could work up the subject very much better than I could.  
Therefore I earnestly, and without any reservation, hope that  
you will proceed with your paper, so that I return your notes.  
You seem already to have well investigated the subject. I  
confess on receiving your note that I felt rather flat at my  
recent work being almost thrown away, but I did not intend  
to show this feeling. As a proof how little advance I had  
made on the subject, I may mention that though I had been  
collecting facts on the colouring, and other sexual differences  
in mammals, your explanation with respect to the females  
had not occurred to me. I am surprised at my own stupidity,  
but I have long recognised how much clearer and deeper  
your insight into matters is than mine. I do not know how  
far you have attended to the laws of inheritance, so what  
follows may be obvious to you. I have begun my discussion  
on sexual selection by showing that new characters often  
appear in one sex and are transmitted to that sex alone, and  
that from some unknown cause such characters apparently  
appear oftener in the male than in the female. Secondly,  
characters may be developed and be confined to the male,

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<sup>1</sup> See Letter 203, Vol. I., p. 283.

Letter 430 and long afterwards be transferred to the female. Thirdly, characters may arise in either sex and be transmitted to both sexes, either in an equal or unequal degree. In this latter case I have supposed that the survival of the fittest has come into play with female birds and kept the female dull-coloured. With respect to the absence of spurs in the female gallinaceous birds, I presume that they would be in the way during incubation; at least I have got the case of a German breed of fowls in which the hens were spurred, and were found to disturb and break their eggs much. With respect to the females of deer not having horns, I presume it is to save the loss of organised matter. In your note you speak of sexual selection and protection as sufficient to account for the colouring of all animals, but it seems to me doubtful how far this will come into play with some of the lower animals, such as sea anemones, some corals, etc., etc. On the other hand Hækel<sup>1</sup> has recently well shown that the transparency and absence of colour in the lower oceanic animals, belonging to the most different classes, may be well accounted for on the principle of protection.

Some time or other I should like much to know where your paper on the nests of birds has appeared, and I shall be extremely anxious to read your paper in the *Westminster Review*.<sup>2</sup> Your paper on the sexual colouring of birds will, I have no doubt, be very striking. Forgive me, if you can, for a touch of illiberality about your paper.

Letter 431

To A. R. Wallace.

March 19th, 1868.

*The Variation of Animals and Plants* having been published on Jan. 30th, 1868, Mr. Darwin notes in his diary that on Feb. 4th he "Began on Man and Sexual Selection." He had already (in 1864 and 1867) corresponded with Mr. Wallace on these questions—see for instance the *Life and Letters*, III., p. 89; but, owing to various interruptions, serious work on the subject did not begin until 1869. The following quotations show the line of work undertaken early in 1868.

Mr. Wallace wrote (March 19th, 1868): "I am glad you have got good materials on sexual selection. It is no doubt a difficult subject. One difficulty to me is, that I do not see how the constant *minute* variations, which are sufficient for Natural Selection to work with,

<sup>1</sup> See *Descent of Man* (1901) p. 402.

<sup>2</sup> *Westminster Review*, July, 1867.

could be *sexually* selected. We seem to require a series of bold and Letter abrupt variations. How can we imagine that an inch in the tail of the peacock, or  $\frac{1}{4}$ -inch in that of the Bird of Paradise, would be noticed and preferred by the female."

In regard to sexual selection. A girl sees a handsome man, and without observing whether his nose or whiskers are the tenth of an inch longer or shorter than in some other man, admires his appearance and says she will marry him. So, I suppose, with the pea-hen; and the tail has been increased in length merely by, on the whole, presenting a more gorgeous appearance. J. Jenner Weir, however, has given me some facts showing that birds apparently admire details of plumage.

To F. Müller.

Letter 432

March 28th [1868].

I am particularly obliged to you for your observations on the stridulation of the two sexes of Lamellicorns.<sup>1</sup> I begin to fear that I am completely in error owing to that common cause, viz. mistaking at first individual variability for sexual difference.

I go on working at sexual selection, and, though never idle, I am able to do so little work each day that I make very slow progress. I knew from Azara about the young of the tapir being striped, and about young deer being spotted;<sup>2</sup> I have often reflected on this subject, and know not what to conclude about the loss of the stripes and spots. From the geographical distribution of the striped and unstriped species of *Equus* there seems to be something very mysterious about the loss of stripes; and I cannot persuade myself that the common ass has lost its stripes owing to being rendered more conspicuous from having stripes and thus exposed to danger.

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<sup>1</sup> We are unable to find any mention of F. Müller's observations on this point; but the reference is clearly to Darwin's observations on *Necrophorus* and *Pelobius*, in which the stridulating rasp was bigger in the males in the first individuals examined, but not so in succeeding specimens. *Descent of Man*, Ed. II., Vol. I., p. 382.

<sup>2</sup> Fritz Müller's views are discussed in the *Descent of Man*, Ed. II., Vol. II., p. 305.



Letter 433

To J. Jenner Weir.

Mr. John Jenner Weir,<sup>1</sup> to whom the following letters are addressed, is frequently quoted in the *Descent of Man* as having supplied Mr. Darwin with information on a variety of subjects.

Down, Feb. 27th [1868].

I must thank you for your paper on apterous lepidoptera,<sup>2</sup> which has interested me exceedingly, and likewise for the very honourable mention which you make of my name. It is almost a pity that your paper was not published in some Journal in which it would have had a wider distribution. It contained much that was new to me. I think the part about the relation of the wings and spiracles and tracheæ might have been made a little clearer. Incidentally, you have done me a good service by reminding me of the rudimentary spurs on the legs of the partridge, for I am now writing on what I have called sexual selection. I believe that I am not mistaken in thinking that you have attended much to birds in confinement, as well as to insects. If you could call to mind any facts bearing on this subject, with birds, insects, or any animals—such as the selection by a female of any particular male—or conversely of a particular female by a male, or on the rivalry between males, or on the allurements of the females by the males, or any such facts, I should be most grateful for the information, if you would have the kindness to communicate it.

P.S.—I may give as instance of [this] class of facts, that Barrow asserts that a male *Emberiza* (?) at the Cape has immensely long tail-feathers during the breeding season;<sup>3</sup> and that if these are cut off, he has no chance of getting a wife. I have always felt an intense wish to make analogous trials, but have never had an opportunity, and it is not likely that you or any one would be willing to try so troublesome an experiment. Colouring or staining the fine red breast of a bullfinch with

<sup>1</sup> For a biographical note see Vol. I., Letter 235.

<sup>2</sup> Published by the West Kent Natural History, Microscopical and Photographic Society, Greenwich, 1867. Mr. Weir's paper seems chiefly to have interested Mr. Darwin as affording a good case of gradation in the degree of degradation of the wings in various species.

<sup>3</sup> Barrow describes the long tail feathers of *Emberiza longicauda* as enduring "but the season of love." *An Account of Travels into the Interior of Southern Africa*: London, 1801, Vol. I., p. 244.

some innocuous matter into a dingy tint would be an analogous case, and then putting him and ordinary males with a female. A friend promised, but failed, to try a converse experiment with white pigeons—viz., to stain their tails and wings with magenta or other colours, and then observe what effect such a prodigious alteration would have on their courtship.<sup>1</sup> It would be a fairer trial to cut off the eyes of the tail-feathers of male peacocks; but who would sacrifice the beauty of their bird for a whole season to please a mere naturalist?

To J. Jenner Weir.

Down, Feb. 29th [1868].

I have hardly ever received a note which has interested me more than your last; and this is no exaggeration. I had a few cases of birds perceiving slight changes in the dress of their owners, but your facts are of tenfold value. I shall certainly make use of them, and need not say how much obliged I should be for any others about which you feel confident.

Do you know of any birds besides some of the gallinacæ which are polygamous? Do you know of any birds besides pigeons, and, as it is said, the raven, which pair for their whole lives?

Many years ago I visited your brother, who showed me his pigeons and gave me some valuable information. Could you persuade him (but I fear he would think it high treason) to stain a male pigeon some brilliant colour, and observe whether it excited in the other pigeons, especially the females, admiration or contempt?

For the chance of your liking to have a copy and being able to find some parts which would interest you, I have directed Mr. Murray to send you my recent book on *Variation under Domestication*.

P.S.—I have somewhere safe references to cases of magpies, of which one of a pair has been repeatedly (I think seven times) killed, and yet another mate was always immediately found.<sup>2</sup> A gamekeeper told me yesterday of analogous case.

<sup>1</sup> See Letter 428.

<sup>2</sup> On this subject see *Descent of Man*, Ed. I., Vol. II., p. 104, where Mr. Weir's observations were made use of. This statement is quoted from Jenner (*Phil. Trans.*, 1824) in the *Descent of Man* (1901), p. 620.

Letter 434 This perplexes me much. Are there many unmarried birds? I can hardly believe it. Or will one of a pair, of which the nest has been robbed, or which are barren, always desert his or her mate for a strange mate with the attraction of a nest, and in one instance with young birds in the nest? The gamekeeper said during breeding season he had never observed a single or unpaired partridge. How can the sexes be so equally matched?

P.S. 2nd.—I fear you will find me a great bore, but I will be as reasonable as can be expected in plundering one so rich as you.

P.S. 3rd.—I have just received a letter from Dr. Wallace,<sup>1</sup> of Colchester, about the proportional numbers of the two sexes in *Bombyx*; and in this note, apropos to an incidental remark of mine, he stoutly maintains that female lepidoptera never notice the colours or appearance of the male, but always receive the first male which comes; and this appears very probable. He says he has often seen fine females receive old battered and pale-tinted males. I shall have to admit this very great objection to sexual selection in insects. His observations no doubt apply to English lepidoptera, in most of which the sexes are alike. The brimstone or orange-tip would be good to observe in this respect, but it is hopelessly difficult. I think I have often seen several males following one female; and what decides which male shall succeed? How is this about several males; is it not so?

Letter 435

To J. Jenner Weir.

6, Queen Anne Street, Cavendish Square, W. [March 6th, 1868].

I have come here for a few weeks, for a little change and rest. Just as I was leaving home I received your first note, and yesterday a second; and both are most interesting and valuable to me. That is a very curious observation about the goldfinch's beak,<sup>2</sup> but one would hardly like to trust it without measurement or comparison of the beaks of several

<sup>1</sup> See *Descent of Man*, Ed. 1., Vol. I., pp. 386-401, where Dr. Wallace's observations are quoted.

<sup>2</sup> *Descent of Man*, Ed. 1., Vol. I., p. 39. Mr. Weir is quoted as saying that the birdcatchers can distinguish the males of the goldfinch, *Carduelis elegans*, by their "slightly longer beaks."

male and female birds; for I do not understand that you yourself assert that the beak of the male is sensibly longer than that of the female. If you come across any acute bird-catchers (I do not mean to ask you to go after them), I wish you would ask what is their impression on the relative numbers of the sexes of any birds which they habitually catch, and whether some years males are more numerous and some years females. I see that I must trust to analogy (an unsafe support) for sexual selection in regard to colour in butterflies. You speak of the brimstone butterfly and genus *Edusa*<sup>1</sup> (I forget what this is, and have no books here, unless it is *Colias*) not opening their wings. In one of my notes to Mr. Stainton I asked him (but he could or did not answer) whether butterflies such as the Fritillaries, with wings bright beneath and above, opened and shut their wings more than *Vanessæ*, most of which, I think, are obscure on the under surface. That is a most curious observation about the red underwing moth and the robin,<sup>2</sup> and strongly supports a suggestion (which I thought hardly credible) of A. R. Wallace, viz. that the immense wings of some exotic lepidoptera served as a protection from difficulty of birds seizing them. I will probably quote your case.

No doubt Dr. Hooker collected the Kerguelen moth, for I remember he told me of the case when I suggested in the *Origin*, the explanation of the coleoptera of Madeira being apterous; but he did not know what had become of the specimens.

I am quite delighted to hear that you are observing coloured birds,<sup>3</sup> though the probability, I suppose, will be that no sure result will be gained. I am accustomed with my numerous experiments with plants to be well satisfied if I get any good result in one case out of five.

You will not be able to read all my book—too much detail. Some of the chapters in the second volume are

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<sup>1</sup> *Colias Edusa*.

<sup>2</sup> *Descent of Man*, Ed. 1., Vol. I., p. 395. Mr. Weir describes the pursuit of a red-underwing, *Triphaea pronuba*, by a robin which was attracted by the bright colour of the moth, and constantly missed the insect by breaking pieces off the wing instead of seizing the body. Mr. Wallace's facts are given on the same page.

<sup>3</sup> *Descent of Man*, Ed. 1., Vol. II., p. 110.

Letter 435 curious, I think. If any man wants to gain a good opinion of his fellow-men, he ought to do what I am doing, pester them with letters.

Letter 436

To J. Jenner Weir.

4, Chester Place, Regent's Park, N.W., March 13th [1868].

You make a very great mistake when you speak of "the risk of your notes boring me." They are of the utmost value to me, and I am sure I shall never be tired of receiving them ; but I must not be unreasonable. I shall give almost all the facts which you have mentioned in your two last notes, as well as in the previous ones ; and my only difficulty will be not to give too much and weary my readers. Your last note is especially valuable about birds displaying the beautiful parts of their plumage. Audubon<sup>1</sup> gives a good many facts about the antics of birds during courtship, but nothing nearly so much to the purpose as yours. I shall never be able to resist giving the whole substance of your last note. It is quite a new light to me, except with the peacock and Bird of Paradise. I must now look to turkey's wings ; but I do not think that their wings are beautiful when opened during courtship. Its tail is finely banded. How about the drake and *Gallus bankiva* ? I forget how their wings look when expanded. Your facts are all the more valuable as I now clearly see that for butterflies I must trust to analogy altogether in regard to sexual selection. But I think I shall make out a strong case (as far as the rather deceitful guide of analogy will serve) in the sexes of butterflies being alike or differing greatly—in moths which do not display the lower surface of their wings not having them gaudily coloured, etc., etc.—nocturnal moths, etc.—and in some male insects fighting for the females, and attracting them by music.

My discussion on sexual selection will be a curious one—a mere dovetailing of information derived from you, Bates, Wallace, etc., etc.

We remain at above address all this month, and then return home. In the summer, could I persuade you to pay us a visit of a day or two, and I would try and get Bates and some others to come down ? But my health is so precarious,

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<sup>1</sup> In his *Ornithological Biography*, 5 vols., Edinburgh, 1831-49.

I can ask no one who will not allow me the privilege of a poor old invalid ; for talking, I find by long and dear-bought experience, tries my head more than anything, and I am utterly incapable of talking more than half an hour, except on rare occasions.

I fear this note is very badly written ; but I was very ill all yesterday, and my hand shakes to-day.

To J. Jenner Weir.

Letter 437

4, Chester Place, Regent's Park, N.W., March 22nd [1868].

I hope that you will not think me ungrateful that I have not sooner answered your note of the 16th ; but in fact I have been overwhelmed both with calls and letters ; and, alas, one visit to the British Museum of an hour or hour and a half does for me for the whole day.

I was particularly glad to hear your and your brother's statement about the "gay" deceiver-pigeons.<sup>1</sup> I did not at all know that certain birds could win the affections of the females more than other males, except, indeed, in the case of the peacock. Conversely, Mr. Hewitt, I remember, states that in making hybrids the cock pheasant would prefer certain hen fowls and strongly dislike others. I will write to Mr. H. in a few days, and ask him whether he has observed anything of this kind with pure unions of fowls, ducks, etc. I had utterly forgotten the case of the ruff,<sup>2</sup> but now I remember having heard that it was polygamous ; but polygamy with birds, at least, does not seem common enough to have played an important part. So little is known of habits of foreign birds : Wallace does not even know whether Birds of Paradise are polygamous. Have you been a large collector of caterpillars ? I believe so. I inferred from a letter from Dr. Wallace, of Colchester, that he would account for Mr. Stainton and others rearing more female than male by their having collected the larger and finer caterpillars. But I misunderstood him, and he maintains that collectors take all caterpillars, large and small, for that they collect the

<sup>1</sup> Some cock pigeons "called by our English fanciers gay birds are so successful in their gallantries that, as Mr. H. Weir informs me, they must be shut up, on account of the mischief which they cause."

<sup>2</sup> The ruff, *Machetes pugnax*, was believed by Montagu to be polygamous. *Descent of Man*, Ed. I., Vol. I., p. 270.

Letter 437 caterpillars alone of the rarer moths or butterflies. What think you? I hear from Professor Canestrini<sup>1</sup> in Italy that females are born in considerable excess with *Bombyx mori*, and in greater excess of late years than formerly! Quatrefages writes to me that he believes they are equal in France. So that the farther I go the deeper I sink into the mire. With cordial thanks for your most valuable letters.

We remain here till April 1st, and then hurrah for home and quiet work.

38

To J. Jenner Weir.

4, Chester Place, N.W., March 27th [1868].

I hardly know which of your three last letters has interested me most. What splendid work I shall have hereafter in selecting and arranging all your facts. Your last letter is most curious—all about the bird-catchers—and interested us all. I suppose the male chaffinch in “pegging” approaches the captive singing-bird, from rivalry or jealousy—if I am wrong please tell me; otherwise I will assume so. Can you form any theory about all the many cases which you have given me, and others which have been published, of when one [of a] pair is killed, another soon appearing? Your fact about the bullfinches in your garden is most curious on this head.<sup>2</sup> Are there everywhere many unpaired birds? What can the explanation be?

Mr. Gould assures me that all the nightingales which first come over are males, and he believes this is so with other migratory birds. But this does not agree with what the bird-catchers say about the common linnet, which I suppose migrates within the limits of England.

Many thanks for very curious case of *Pavo nigripennis*.<sup>3</sup> I am very glad to get additional evidence. I have sent your fact to be inserted, if not too late, in four foreign editions which are now printing. I am delighted to hear that you

<sup>1</sup> See *Descent of Man* (1901), p. 385.

<sup>2</sup> Mr. Weir stated that at Blackheath he never saw or heard a wild bullfinch, yet when one of his caged males died, a wild one in the course of a few days generally came and perched near the widowed female, whose call-note is not loud. *Descent of Man* (1901), p. 623.

<sup>3</sup> See *Animals and Plants*, Ed. II., vol. I., p. 306.

approve of my book ; I thought every mortal man would find the details very tedious, and have often repented of giving so many. You will find pangenesis stiff reading, and I fear will shake your head in disapproval. Wallace sticks up for the great god Pan like a man.

Letter 438

The fertility of hybrid canaries would be a fine subject for careful investigation.

To J. Jenner Weir.

Down, April 4th [1868].

I read over your last ten (!) letters this morning, and made an index of their contents for easy reference ; and what a mine of wealth you have bestowed on me. I am glad you will publish yourself on gay-coloured caterpillars and birds ;<sup>1</sup> it seems to me much the best plan ; therefore, I will not forward your letter to Mr. Wallace. I was much in the Zoological Gardens during my month in London, and picked up what scraps of knowledge I could. Without my having mentioned your most interesting observations on the display of the Fringillidæ,<sup>2</sup> Mr. Bartlett told me how the Gold Pheasant erects his collar and turns from side to side, displaying it to the hen. He has offered to give me notes on the display of all Gallinacæ with which he is acquainted ; but he is so busy a man that I rather doubt whether he will ever do so.

I received about a week ago a remarkably kind letter from your brother, and I am sorry to hear that he suffers much in health. He gave me some fine facts about a Dun Hen Carrier which would never pair with a bird of any other colour. He told me, also, of some one at Lewes who paints

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<sup>1</sup> See *Descent of Man*, Ed. I., vol. I., p. 417, where Mr. Weir's experiments are given ; they were made to test Mr. Wallace's theory that caterpillars, which are protected against birds by an unpleasant taste, have been rendered conspicuous, so that they are easily recognised. They thus escape being pecked or tasted, which to soft-skinned animals would be as fatal as being devoured. See Mr. Jenner Weir's papers, *Transact. Entomolog. Soc.*, 1869, p. 2 ; 1870, p. 337. In regard to one of these papers Mr. Darwin wrote (May 13th, 1869) : "Your verification of Wallace's suggestion seems to me to amount to quite a discovery."

<sup>2</sup> *Descent of Man* (1901), p. 738.



Letter 439 his dog! and will inquire about it. By the way, Mr. Trimen tells me that as a boy he used to paint butterflies, and that they long haunted the same place, but he made no further observations on them. As far as colour is concerned, I see I shall have to trust to mere inference from the males displaying their plumage, and other analogous facts. I shall get no direct evidence of the preference of the hens. Mr. Hewitt, of Birmingham, tells me that the common hen prefers a salacious cock, but is quite indifferent to colour.

Will you consider and kindly give me your opinion on the two following points. Do very vigorous and well-nourished hens receive the male earlier in the spring than weaker or poorer hens? I suppose that they do. Secondly, do you suppose that the birds which pair first in the season have any advantage in rearing numerous and healthy offspring over those which pair later in the season? With respect to the mysterious cases of which you have given me so many, in addition to those previously collected, of when one bird of a pair is shot another immediately supplying its place, I was drawing to the conclusion that there must be in each district several unpaired birds; yet this seems very improbable. You allude, also, to the unknown causes which keep down the numbers of birds; and often and often have I marvelled over this subject with respect to many animals.

Letter 440

To A. R. Wallace.

The following refers to Mr. Wallace's article "A Theory of Birds' Nests," in Andrew Murray's *Journal of Travel*, Vol. I., p. 73. He here treats in fuller detail the view already published in the *Westminster Review*, July, 1867, p. 38. The rule which Mr. Wallace believes, with very few exceptions, to hold good is, "that when both sexes are of strikingly gay and conspicuous colours, the nest is . . . such as to conceal the sitting bird; while, whenever there is a striking contrast of colours, the male being gay and conspicuous, the female dull and obscure, the nest is open, and the sitting bird exposed to view." At this time Mr. Wallace allowed considerably more influence to sexual selection (in combination with the need of protection) than in his later writings. The following extract from a letter from Mr. Wallace to Darwin (July 23rd, 1877) fixes the period at which the change in his views occurred: "I am almost afraid to tell you that in going over the subject of the colours of animals, etc., etc., for a small volume of essays, etc., I am preparing, I have come to conclusions directly opposed to voluntary sexual selection, and

believe that I can explain (in a general way) all the phenomena of sexual Letter 440 ornaments and colours by laws of development aided by simple 'Natural Selection.'" He finally rejected Mr. Darwin's theory that colours "have been developed by the preference of the females, the more ornamented males becoming the parents of each successive generation." *Darwinism*, 1889, p. 285. See also Letters 442, 443, 449, 450, etc.

Down, April 15th, [1868].

I have been deeply interested by your admirable article on birds' nests. I am delighted to see that we really differ very little,—not more than two men almost always will. You do not lay much or any stress on new characters spontaneously appearing in one sex (generally the male), and being transmitted exclusively, or more commonly only in excess, to that sex. I, on the other hand, formerly paid far too little attention to protection. I had only a glimpse of the truth; but even now I do not go quite as far as you. I cannot avoid thinking rather more than you do about the exceptions in nesting to the rule, especially the partial exceptions, *i.e.*, when there is some little difference between the sexes in species which build concealed nests. I am not quite satisfied about the incubating males; there is so little difference in conspicuousness between the sexes. I wish with all my heart I could go the whole length with you. You seem to think that male birds probably select the most beautiful females; I must feel some doubt on this head, for I can find no evidence of it. Though I am writing so carping a note, I admire the article thoroughly.

And now I want to ask a question. When female butterflies are more brilliant than their males you believe that they have in most cases, or in all cases, been rendered brilliant so as to mimic some other species, and thus escape danger. But can you account for the males not having been rendered equally brilliant and equally protected? <sup>1</sup> Although it may be most for the welfare of the species that the female should be protected, yet it would be some advantage, certainly

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<sup>1</sup> See Wallace in the *Westminster Review*, July, 1867, p. 37, on the protection to the female insect afforded by its resemblance either to an inanimate object or to another insect protected by its unpalatableness. The cases are discussed in relation to the much greater importance (to the species as a whole) of the preservation of the female insect with her load of eggs than the male who may safely be sacrificed after pairing. See Letter 189, note 3.